

## MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—THE JUSTIFICATION OF ATTITUDES <sup>1</sup>

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I AM going this evening to discuss a subject of some obscurity : the sense in which, as we say, people are more or less *justified* in having attitudes of certain sorts and of certain degrees of intensity towards objects and situations of certain sorts, and in certain typical constellations of circumstances. It is obvious that talk of this sort is widespread and well-established : we have no hesitation in saying that someone is showing a thoroughly warranted attitude of amazement or anger or shame in a certain situation, or that he is manifesting such an attitude in a proper and suitable degree, whereas, in other cases, we have no hesitation in finding an attitude bizarre, uncalled for, out of place, unwarranted, or at least of quite an inappropriate quality or intensity. Our use of such pejorative and laudatory qualifications is by no means confined to attitudes of a pretentious cast, in which rational reflexion plays a considerable part, and which are informed by various lofty aspirations. It may be possible to be justified or unjustified in a deliberate decision, a moral valuation or a critical aesthetic preference, but it is just as possible to be justified or unjustified in a transient spasm of envy, of jealousy, of vindictive wrath or of tremulous self-abasement. Ordinary speech undoubtedly recognizes a propriety and a validity in our emotional and other attitudes which has seemed very questionable to philosophers : our task this evening is to look into our usage on this point, and see whether

<sup>1</sup> Based on papers given at the Cambridge Moral Science Club and at University College, London.

ordinary speech may not have a better justification for talking about the justification of attitudes than many philosophers have supposed. The theme I am going to discuss is one that has interested me ever since I read Brentano's *Vom Ursprung sittlicher Erkenntnis*, a work which first put forward the doctrine of an inner moment of *Berechtigung* in our emotional attitudes, something analogous to, but different from the *Evidenz* that informs certain of our judgments. It is also a doctrine interestingly developed in Meinong's neglected theory of emotional presentation, as in Scheler's elaborate emotional *a priori* or 'logic of the heart', which builds a huge structure upon an exciting phrase of Pascal's. From thence it has spread into an underworld of influential but badly written books, which I shall not here try to enumerate. But my own poor attempts to contribute to this 'logic of the heart' seem to have fallen on to a singularly stony and unreceptive soil: I don't seem to have convinced anyone that there is something really worthwhile and illuminating to be said on the matter. I therefore return to the attack, though I shall deal with my topic in a rather different manner than heretofore.

Before discussing the appropriateness of attitudes, I shall say something about the bare bones of my subject, the degree of formal complexity that it involves. It is plain that we normally have *three* things to inquire into in discussing the appropriateness of an attitude, the *sort* of attitude under consideration, in which I wish to include both its characteristic pattern or make-up, and also its intensity, the *object* or *objects* of our attitude, the sort of things with which it is concerned or upon which it discharges itself, and lastly the whole *background of circumstances* in which an attitude arises, whether this be actual and present, or merely imagined and foreshadowed. It seems clear that we sometimes do recognize cases of appropriateness in attitude which have a degree of logical complexity less than this. Thus the Stoics and many other detached persons have held that there was some attitude of apathy or serene good cheer or what not, which is *always* appropriate whatever our object, and whatever the circumstances; this means that the attitude in question is justifiable *per se*, without regard to anything external. In the same way, some have held that we ought to adopt *some* attitude, it does not matter what, towards certain important objects: everything can be tolerated except that Laodicean mean which is really the absence of any attitude at all. And many would hold, of course, that there are attitudes to certain solemn duties, or to certain moral enormities, which

are always justifiable or unjustifiable, without *any* consideration of circumstances. But it seems plain that the instances we have adduced, if they are genuine instances at all, are all either eccentric or degenerate: the justifications that we normally recognize are always justifications of attitudes more or less definite in type and intensity, towards objects more or less definite in character, in circumstances similarly definite. We may note, further, that the justifications we acknowledge are themselves capable of degree: they have an upper limit in *complete justification*, from which they may fall short in a large number of distinct degrees. And we may note, further, that there is an ambiguity in the word 'unjustified' which connotes such falling short: it may be merely *privative* in meaning, pointing to some absence or partial absence of positive justification, or it may, on the other hand, express a sort of *counter-justification* or justification in reverse. An attitude of collapsing mirth will be unjustified in the former sense by the fact that it is spring and that I am walking agreeably by the Cam: it will be unjustified in the latter sense by the fact that I find myself in Westminster Abbey, and that the body of some sainted king is the main object in the proceedings.

All this rehearsal of mere grammar is too Aristotelian and too Meinongian to be further pursued: we must try to say something about the analysis of the factors involved in such justificatory situations. In regard to attitudes, we accept an analysis which identifies them, in part at least, with certain more or less definite *policies of action*, policies which could, for instance, be characterized as policies of withdrawal, of self-obliteration, of aggressive lashing out, of nauseated expulsion, of tranquil basking, of luxuriant gloating, of prurient exploration, of limp breakdown and so forth, and which cover in their range of expression a vast number of fully fledged performances, as well as countless variously truncated symbolic gestures. This does not mean that the attitudes in question may not be *introspectively* as well as *extraspectively* evident, for those about to behave in any out of a certain range of possible ways, are also, for the most part, ready for the varied experiences which make up the inner or felt side of such behaviour: the readiness for such experiences, as well as the readiness for certain sorts of action, give meaning to the reports that they then utter. The same words 'I am furious' are at once a betrayal of our readiness for certain smashing and retaliatory acts and gestures, as well as for our readiness for the varied experiences which are what we feel like when such acts and gestures are executed. The only thing I

am flatly denying, as a supposition 'profitless and unintelligible, is the doctrine that there are certain unanalysable nuances of inner mood corresponding to our various attitudinal names. I repudiate this because it makes nonsense of the whole connexion between attitudes as we feel them, and attitudes as they show themselves in action, which has then to be treated either as an extraordinary empirical accident, or as yet another *ad hoc* case of the synthetic *a priori*. I repudiate it also, because it is false to the whole range of our normal introspective diction, in which a man spontaneously describes his attitude in such words as: 'I felt ready to burst', 'I felt ready to collapse', 'ready to swear', and so forth.

If we thus connect attitudes, in part at least, with peculiar active readinesses, we may pass on to that entirely traditional conception of their relation to their objects which looks upon this relation as *causal*, the object in its setting of circumstances being that which provokes, occasions or arouses some attitude in us. To this we must add the obvious emendation that it isn't really the object as such, which may be remote, inaccessible or even non-existent, but rather our own thought of, or belief in, such an object which evokes the given attitude. It will, however, be plain, on reflexion, that causation alone isn't sufficient to explain a relation of genuine *concern* with an object: otherwise the mere fact that my wife has annoyed me at breakfast, and that I vent my wrath on an unoffending student, will mean that my wrath is in this case directed at my wife. The relation of concern between attitude and object is in no sense mystical or hard to understand: it involves no more than that the actions for which we are prepared, and for which we feel ourselves prepared, are all actions *physically* bound up with a certain object. Thus my passionate love for you is a readiness for embraces and caresses to be lavished, not on dogs or mistresses or old furniture or saintly relics, but on you, it is a readiness for protective poses or clinging poses or mastering poses or surrendering poses, not connected promiscuously with persons in general, but exclusively with you. In the same way the attitude expressed by 'What couldn't I do to a Worthington' (or 'a beefsteak', or 'a brace of partridges' or 'an Abyssinian village') is not at all mysteriously connected with its object. There is in fact no problem more completely gratuitous than the one somewhere raised by Professor Broad, as to how, if we are thinking of two objects simultaneously, and experiencing distinct attitudes to each of them, we can be at all sure which attitude is directed to what object, that we are not perhaps really

pitying a torturer and condemning his victim when we seem to ourselves to be doing the opposite. If there is any exposure of the utter inadequacy of the inner-quality theory of attitudes, it lies in the sort of example we have cited.

So far there is little in our account of the relation of attitudes to their objects and objective conditions, which leaves scope for the notion of a justifiability or an appropriateness of such attitudes. The mere fact that a policy is excited or modified by our thought of certain things and circumstances, and that this policy itself issues in acts physically bound up with such objects, will not give sense to the assertion that the policy in question was *justified* by the character of its objects and circumstances, or that it was *appropriate* to them. At best we could predicate non-justification of an attitude in a derivative sense, if the attitude was founded on false opinion, on some mistaken view of its object. If I love you for your kindness, purity and personal fondness for me, and you turn out to be hard of heart, libidinous and incapable of personal attachment, then my love and devotion will be to this extent unjustified, that they are founded on false assumptions: if I choose, however, to love you for your Dolores-like qualities, there seems no plain sense in which such a love can be unjustified. The justification we are considering is in fact only a justification in so far as the *real* state of things is taken into account: what we should *like* to give a sense to is a justification for loving people who *seem* to us kind, pure and fond of us, not for the mere belief that people really *have* the properties that determine us to love them. So far as we can see, such a justification is not at all easy to come by, unless our statement that a certain attitude is justified is merely an expression of a further attitude of recommendation or sponsorship which is directed to the first attitude. Doubtless there are occasions on which this is all there is to the matter, but these are not the cases we are now considering. It would not even seem, *prima facie*, that men uniformly have similar attitudes to the same objects in the same circumstances, and much less that they *must* do so. We could, with a little effort, follow Francis Galton, and attach our emotions to the most ludicrously inappropriate objects: we could learn to sweat with fear before a horse, or to direct our anger to things bald or snub-nosed or viviparous or parallelepipedal; we might teach ourselves to feel guilt whenever we blew our noses or whenever anyone else did so. Any sentiment could thus come to be attached to any object, and the whole issue of rationality or justifiability would become a wholly confused and senseless one.

There is, however, one obvious way in which we can give our attitudes a genuine justifiability or unjustifiability, and that is to make it part of their *definition* or *description* to be concerned with objects presumed to be of certain sorts, and presumed also to be surrounded by certain types of circumstance. We can thus, by a mere exercise of the sovereign rights of Humpty Dumpty, make it part of what we mean by certain attitudinal names that the attitudes named by them should be capable of being excited by the thought of certain objects, and that they should also terminate in acts physically connected with such objects: we can also make it part of what we mean by certain attitudes that they should be strengthened or weakened by an idea of, or by a belief in, certain typical circumstances or consequences. All this means that we can make it 'of the essence' of certain sorts of attitudes that they should be amenable to certain kinds of reasons and counter-reasons, by means of which they may be confirmed or tested, whereas other reasons, however weighty and impressive, will not contribute a jot to such justification or testing. We can, if we like, say that certain attitudes involve judgments or supposals, by the truth or falsehood of which they may be validated or invalidated: this would, however, be to recur to that old intellectualistic tradition according to which fear always involves a confused judgment of danger, desire a confused judgment of profit, and so on. Quite obviously, this would involve a particularly bad use of the much abused word 'judgment', since nothing seems more utterly absent from many of our emotional attitudes than a considered pronouncement on, or assessment of anything. There would, however, be less objection to our following a reputable Austrian tradition, which says that our attitudes have certain cognitive *presuppositions*, especially if we meant by this latter that they are such as to respond to certain presented features of objects, and to be modified by certain characteristic considerations. The having of such presuppositions would *not* need to be anything actual nor discoverable by introspection: it would show itself rather in the whole pattern of reflexion, the whole marshalling of reasons and counter-reasons, by means of which such attitudes would be confirmed or tested. The presuppositions in question could be said to weigh heavily in our attitude according as certain discoveries led to a considerable modification in our policies, or according to the low degree of confidence in a possibility which was sufficient to effect such a modification. If one possibility dimly entertained made a much bigger difference to our policies, than another confidently believed or known, then

there might be said to be a much stronger presumption of the former possibility in our attitude than of the latter. Our attitude would always essentially be an attitude *towards* this or that possibility, and it would lose its whole sense and its *raison d'être* if the possibility in question proved really chimerical. It will be noted that we avoid circularity in the account we are giving by speaking of the modifications of *attitudinal policy* effected by the discoveries which correspond to certain cognitive presuppositions: if we said that *the attitude itself* was modified by such discoveries, we should be forgetting that the presuppositions in question were a part of itself.

A question now presents itself which is itself a question of justification: Are we justified in incorporating into what we mean by having certain attitudes, the presence of presuppositions which enable us to attribute justifiability to the attitudes in question? Isn't this merely an elaborate method of cheating? Aren't we trying to give an impression of rigour and objectivity, after everything questionable has been smuggled, Spinoza-fashion, into our axioms and definitions? Isn't our procedure like trying to prove that men can't die, by simply deciding that, if any of our friends or acquaintances does manage to pass away, we shall meet the situation by ruling him not to have been a man? It is true that physics is honeycombed by similar proceedings, which confer the august flavour of the *a priori* on certain purely contingent causal relations, but the bad philosophy thus engendered makes them very much *not* to be recommended. Stevenson has, in fact, developed two techniques, one that does, and one that does not, incorporate certain objective presuppositions into the meaning of the name for an attitude, in his two distinct models for the meaning of 'good'. In the one model the use of 'good' merely says that we have a certain policy of approval, and also expresses a wish to communicate such a policy to others: we may then support our utterance with any reasons which actually prove effective in propagating this policy. In the other model, the content of certain reasons becomes part of the meaning of the word 'good', so that it becomes analytically true that it is good to promote the greatest happiness of the greatest number, or to mortify the flesh, or to assimilate oneself to God, and so forth. By practising the incorporation in question one then lends covert support to the ideals of Christianity, or of democracy, or of asceticism, or of some other 'ism': one redirects the attitudes connected with the word 'good' to the peculiar objects one puts in one's definition. Stevenson calls the device embodied in this second

model that of 'persuasive definition', and though he *says* that there is nothing discreditable about it, his treatment is such as to attach a definite stigma to it. It creates an illusory appearance of objectivity and validity where no such objectivity is really present : in so doing it sins against those analytic values to which Stevenson and his kind are so exclusively devoted. For while it is theoretically just as open to a man to reject some definition of 'good', as to reject any particular reason we bring forward for saying that something is good, yet the whole process of persuasive definition makes such a rejection more difficult : it tricks a man into thinking that he is committed to saying something that he isn't really bound to say at all, and it makes it seem absurd to deny something, which he really can deny with impunity. Is there any good reason why we should thus cover up an arbitrary choice or an empirical connexion, by words that give them this flavour of a *priori* necessity ?

It seems to me that there are two reasons why we should *sometimes* do this. The first is the fact that there is an extraordinarily close empirical connexion between certain typical policies of action and certain features of objects that are their normal excitants. It simply is not the case, for instance, that people are very often in a mood in which they are disposed to lash out indiscriminately at *any* and *every* object, though Malays and maniacs may occasionally be so : people normally lash out only at *provocative* objects, and though it isn't easy, it is by no means impossible to give a comprehensive account of the varied forms of obstruction, resistance, intrusion, persecution, effrontery, baiting, belittling, taunting, flouting and so forth in which provocation may be held to consist. It is *sub specie provocationis* that objects anger us or annoy us, and not merely because they are loud or persistent or unfriendly or immovable or what not. It is, moreover, an empirical fact that, wherever we are in a state which allows us to be influenced by factual considerations, our lashing out policy does tend to be damped down if it becomes really plain that there is nothing provocative in the thing or situation before us, that no one is really obstructing, resisting, baiting, flouting or lording it over us, whereas it may be reinforced if it becomes plain that someone or something is genuinely being provocative. A creature which, when it does lash out at anything, *never* pays the slightest heed to the sort of thing it is lashing out at, and which never cares whether anything is or is not being provocative, would be a rare bird altogether, and is certainly not one that I myself have met or heard of. Much the same might be said of our other attitudes :

the varied forms of mourning grief, with their policy of prostration, sinking, retardation, indifference to immediate calls, and unpractical dwelling on things past and done for, is connected, almost invariably, with the destruction, removal, degradation or injury of something we have regarded with another attitude known as cherishing affection: I have yet to hear of a man who grieved over something that he did not cherish, or that he didn't imagine to be lowered, or lost, or injured, or destroyed, and whose grief was not at all weakened by the discovery that the thing in question wasn't really harmed at all, or by a change of heart which made it no longer seem worthy of his affection. Even the lunatics in their asylums grieve over things marvelously precious, that have been sadly sullied or irreparably impaired, though they might be hard put to it to say exactly what they are thus grieving over. So much is this so that Dr. Ewing has somewhere suggested that we must have a glimmer of *a priori* insight into the tendency of certain types of situation to awaken certain attitudes in us.

We may now ask ourselves a question: granted that certain policies of action almost invariably go together with certain objective presuppositions and presumptions, and are regularly affected by certain discoveries, is it none the less our linguistic duty to provide for the mere possibility that there might be beings otherwise constituted than ourselves, and should we therefore refuse to incorporate into the meaning of our attitudinal names a reference to such objective presuppositions as would lend them an impersonal validity? I can't for the life of me see why we should be obliged to do so. It runs counter to our normal linguistic policies that we should refuse to cover with a single blanketing name features that almost invariably go together, and that we should take elaborate pains to provide for contingencies that aren't ever likely to arise, and which are, in some cases, merely logical possibilities. Surely, if we must have words to cover such strange contingencies, they should be queer words belonging to the technical vocabulary of philosophers, and not ordinary words whose purpose is so different? We have, for instance, well established ways of talking about those sounds which, in our experience, invariably arise in connexion with vibrating objects and media: should we also feel bound to provide for those other logically possible 'sounds and sweet airs' which are without concrete source or provenance whatsoever? Surely it would be highly misleading to provide ordinary linguistic pigeonholes for situations that seldom or never arise? It might lead us to think that what was infrequent

or even wholly non-existent, was quite the regular run of things. And it is plain, in fact, that the way in which many modern philosophers *have* chosen to talk about their feelings, has blinded their eyes to many obvious truths about them.

But there is yet another reason why we should *not* refuse (in certain cases at least) to incorporate certain characteristic claims and presuppositions into the meaning of our attitudinal names, and that is the fact that such claims and presuppositions are *in fact* firmly incorporated into those meanings, and that we should be using such names in a wholly new and confusing sense if we tried to leave such presuppositions out. Thus it is certainly part of what we ordinarily mean by 'jealousy' that it should be directed to someone enjoying favours from someone that we should very much prefer were directed to ourselves: to talk of a jealousy that one might direct to the number of particles in the universe, or to the restrictive practices of the Board of Trade would be to use the word in a wholly meaningless or self-contradictory fashion. It is likewise part of what we ordinarily mean by 'ethical' or 'moral' approval, that it should proceed according to rule, and that it should be directed to the free acts of persons, and to what are regarded as their bearing upon certain other personal interests: it simply would not be clear what was meant by an ethical approval directed to the weather, or to one's fiancée's charming ways, or to the use of thatch in covering houses. The whole proposal that we should leave out all presuppositional meaning in laying down the meaning of an attitudinal name like 'jealousy' or 'ethical feeling', would have highly inconvenient consequences. It might lead us to think that there was something unique and unanalysable about the feelings in question, than which nothing can be further from the case. And it might also lead us to think that it was a remarkable empirical coincidence, or a plain case of the synthetic *a priori*, that certain attitudes were so uniformly connected with certain sorts of objective circumstance. It would seem a wonder that men could feel ethical approval only of voluntary acts, or that they never were jealous except in cases where love was involved. And if we did at times succeed in banishing all this presuppositional meaning from our attitudinal names, we might still find ourselves slipping back into the old senses at other times. Thus even Stevenson, who deliberately uses the word 'ethical' to cover our approval of a beefsteak or a bedstead, as much as of the social aspects of voluntary activity, none the less chooses practically all his examples from the latter sphere, and has a long chapter on avoidability which is entirely

out of place if 'ethical approval' is to mean just what he wants it to mean. For we don't refuse to approve of a graceful carriage, nor to disapprove of epilepsy, and to express our approval or our disapproval in the use of the terms 'good' and 'bad', merely because the conditions in question are unavoidable, and are not to be altered by personal choice or suasion. We certainly call a graceful carriage a good thing, and epilepsy a bad thing, and we invite the world to share this preference, even if we think that neither condition will be modified by our judgments. It is only that special sort of approval which the ordinary man (not Stevenson) calls 'ethical' or 'moral', which is confined to cases of avoidable activity, and this special sort of approval Stevenson declares to be wholly lacking in theoretical interest, inasmuch as it exhibits no peculiar methodological features. By using the term 'ethics' in the title of his book, Stevenson has deceived himself and others into thinking that he has made an important contribution to ethical literature, whereas he has in fact ignored or minimized practically all the distinctive features of ethical methodology. If proof is required that it is a bad thing to use attitudinal names in a radically new and presuppositionless manner, it is to be found in the extraordinary confusions which surround Stevenson's treatment and influence. It was important that such a book should have been written: it is also important that the performance should not be repeated.

If we then hold that it is sometimes right and proper to incorporate certain objective presuppositions into the meaning of our attitudinal names, the question arises *when* it is important to do so. Here I have no general recommendation to make. The advisability depends on the extent to which, in fact, certain presuppositions and aspirations normally go together with certain attitudinal policies, so as to constitute an important standing concomitance that deserves special linguistic recognition; it also depends on the extent to which such standing concomitances have *in fact* been recognized in our language. Different recommendations would be appropriate in different cases. In the attitudes covered by such names as 'envy', 'contempt', 'admiration', 'reverence' and so forth, there are fairly clear-cut presuppositions, connected with the notions of superiority and inferiority, which it would be absurd to leave out: so uniform, in fact, are they, that there would be small point even in trying to distinguish between different species of such attitudes. But in other cases attitudinal names cover much vaguer presuppositions and much wider variation in

policy, and here many quite different recommendations would be equally legitimate.

I don't, however, wish to range over the whole field of possible human attitudes, but rather to say something about a certain family of attitudes, once highly regarded but now somewhat neglected, which seem to me of very central importance in the regulation and direction of human life. These are the attitudes that one might group together under such vague titles as 'reasonable', 'dispassionate', 'impartial', 'disinterested' and the like: they represent, in some sense, a determined attempt to steer our attitudinal policies *away* from anything that is merely personal, contingent, arbitrary, provisional or ill-considered. I shall avoid the vagueness and the associative richness of old words, and shall refer to them all as 'Butlerian attitudes', since they have many of the characteristics attributed by Butler to his conscience. Perhaps I should here conflate the two major moralists of the eighteenth century, and refer to them all as Butlerian-Smithian attitudes: what I mean by such attitudes will not, however, be impenetrably obscure, nor will it be doubted that they form an all-important segment in our attitudinal life. By Butlerian attitudes I mean attitudes that are either entirely of higher order, or which involve higher order components: to preside and govern is part of their 'idea', they are not so much concerned with external situations as with our policies towards these latter, which they always seek to organize and to bring into line. In so far as my attitude is Butlerian, I demand that my first-order policies falling within a certain segment should be all subject to certain general prescriptions; and in so far as I opt for some definite policy, I may be said further, to claim implicitly that this policy *is* in harmony with the prescriptions in question. In so far as my attitude is Butlerian I am prepared, further, to organize *other* people's policies and activities as much as my own: my prescriptions might be put in the first person plural in some such form of words as 'Let us all be thus and thus disposed to such and such objects in this or that sort of situation'. It is not of course needful that these claims and demands should be voiced in my overt utterances: they will show themselves rather in the way in which I bully myself and others whenever I note some sign of backsliding or halfheartedness, as well as in the way in which I seek to modify such policies as are plainly at variance with the prescriptions in question. We may, in the second place, make it part of what we are to mean by a Butlerian attitude, that it should always be, in however small a way, a principle of reflex-

ion ; that it should yield itself to the arbitrament of facts, and to such probabilities as arise out of those facts, and that it should do so without reserve, and that it should also be unwilling to rest until it has pushed its inquisition into such facts and probabilities to the utmost limit of what is available and accessible. In so far as my attitude is a Butlerian one, it can never be indifferent to me whether I can or cannot give reasons for my policies, nor can I ever feel justified in adopting a policy which I don't implicitly claim would still be my policy after the fullest dwelling on the available data. We may, in the third place, make it part of the meaning of a Butlerian attitude, that it seeks to proceed in a manner which is *general* or *impartial* as between case and case, or as between occasion and occasion, or as between person and person : as I have said, I am here conflating Smith with Butler, and perhaps adding a spice of Kant to the mixture. Now I don't want to suggest that Butlerian attitudes are anything peculiarly clear-cut and uniform in human experience and behaviour, much less that I have characterized them in a wholly luminous and unambiguous fashion. Quite plainly many of our attitudes are only approximately Butlerian, *e.g.* the attitude of a chairman pronouncing some authoritative ruling, or that of a tribesman speaking to and for his tribe. And many of our attitudes only satisfy *some* Butlerian conditions : if I try, *e.g.*, to impose some purely personal policy on the world at large, or if I turn my back deliberately upon certain large ranges of fact. There are also very many quite different kinds of Butlerian attitude, which correspond to the different ways or senses in which one might try to be unbiased or impartial. There is an impartiality of universalized bias which subordinates everything to some arbitrarily chosen occasion, person, position, or point of view ; there is, likewise, a negatively indifferent species of impartiality, which hands out a blank charter of liberty and equality to every occasion, position, person and point of view (as in the Protagorean attitude to sense-perception, or as in the *laissez-faire* attitude to our economic activities) ; there are also all those countless forms of impartiality which accept definite but wholly different standards of parity, in their assessment and grading of occasions, positions, persons and points of view. It is certainly impossible to exhaust the ways or senses in which a man's attitude might be said to be impartial, nor is it even the case, that all these ways tend uniformly, in every field of discussion, to one single limit of absolute or maximal impartiality. It seems clear, however, that we do meet with Butlerian attitudes, of different

kinds and of different degrees of stringency, in many fields of human activity, and that we even at times feel ourselves in a position to say of one such attitude that it is *more* Butlerian than another. Thus the impartiality which adopts *some* positive general standard in the assessment of its objects, would appear to be more genuinely Butlerian, more free from arbitrary partiality, than one which is merely negatively indifferent to those objects, or which makes everything centre about some arbitrarily chosen individual fulcrum. However this may be, there are undoubtedly attitudes in the ethical field that one might call attitudes of Butlerian conscientiousness: in these there is a studied attempt to achieve general law-giving, complete openness to facts and probabilities, as well as complete neutrality and unbiased impartiality in regard to personal interests, however vague and ambiguous this last requirement may turn out to be. There are likewise, in the theoretical sphere, attitudes of reasoned expectation, which are in every respect Butlerian, even though their impartiality shows itself in regard to characters and alternatives, to frequencies of occurrence and to ranges of possible variation, rather than in regard to persons and their interests. And it would not be hard to point to cases of Butlerian disinterestedness in the aesthetic sphere, as well as to instances of Butlerian religiosity. Now in so far as a Butlerian component enters into some attitude, that attitude becomes liable to counter-justification as soon as we can discover in it any lack of general applicability, any ignoring of available data, or any trace of bias and partiality. That such conditions involve a great deal that is vague and of manifold interpretation, makes no difference to the fact that we often can and do apply them.

It is not, however, my aim to blaze a trail through the jungle of Butlerian attitudes: I wish rather to raise one general point of method. I wish to ask whether we are justified in having special terms in our language to stand for Butlerian attitudes, and whether we are also justified in making use of these *same* terms, perhaps with superadded qualifications, for attitudes that fall short of Butlerian standards in a number of ways. Should we, for instance, follow traditional moral philosophers in making use of the term 'moral consciousness' to stand for a family of rather stringent Butlerian attitudes, and then also employing the same term, perhaps with some qualification such as 'undeveloped' or 'distorted', of the attitudes expressed in certain of the prescriptions of Nietzsche, or in the exhortations of the Mau Mau or similar movements. Or should we simply throw this traditional approach overboard, and adopt another quite

different policy, that might be called a policy of 'minimum meaning', according to which we shall give our attitudinal names only such meaning as is genuinely common to the whole range of instances of their use, which means in this case that we should decline to see anything fundamental or distinctive in the peculiar features of Butlerian attitudes? It seems to me that there is a great deal to be said on either side.

In favour of the policy of minimum meaning, it may be argued that the traditional policy tends to warp our view of the wide variety of attitudes, some non-Butlerian and some only partially Butlerian, which are covered by such names as 'ethical', 'aesthetic', 'religious', 'scientific' and the like. It sees them all merely as leading up to members of one narrow class of attitudes, themselves in many ways the product of artificial sophistication: it warps our view even of the variety within that narrow class itself. It may, in the second place, be argued that our procedure amounts to a form of dishonest propaganda: we try to trick people into adopting certain attitudes, not by frank advocacy nor by open suasion, but by persuading them that they have always covertly accepted them. It may be objected, further, that the traditional policy really makes it unjustifiable to predicate unjustifiability of such attitudes as lie behind the quasi-moral adjurations of the Mau Mau or the prescriptions of Nietzsche. Such persons certainly were not trying to be Butlerian, so we can't rebuke them for failing to be so. At best we can say that their utterances aren't really moral utterances at all, but this is not to say that they are either unjustifiable or immoral. And a final objection is that, if we continue to pack more and more presuppositional meaning into our attitudinal names, we may end in a position in which no attitude can be called moral which does not accord with a particular moral code, no attitude be called aesthetic or scientific which does not obey certain narrow canons, and so on in other cases. Here surely we may see the apogee of arbitrary unreason madly posing as reason.

Despite all these weighty arguments, I still think that a good case can be made out for the traditional policy. I think it plain that the existence of Butlerian attitudes should be emphatically recognized in our language: without doubt we should have special words to stand for them. The passage from such attitudes as are merely personal and capricious, to those which try to legislate for all and which address themselves to all, and which endeavour to take impartial account of every fact, every possibility, every interest and every point of view, is surely a step of

immense moment: traditional philosophers were not wrong in seeing in it the principal difference between man and other creatures. Man certainly may be said to be a Butlerian animal. And we may say, further, that, in a sense, *reasoned argument* upon matters of aesthetic, scientific, religious or ethical acceptability, can only begin once this big step has been taken: before that there can be nothing but wrangling, and interchange of propaganda. To have no special name for the big step in question may lead to the regrettable consequence that we shall cease to be alive to its momentous character, and that we shall feel that the question 'Shall I be Butlerian or not?' is really one that can come before us for rational decision. Whereas Butlerian attitudes are just those attitudes in which we try to do and be all those things in which being reasonable can be held to consist, so that it would be absurd to treat the adoption of a Butlerian attitude as something that could be rationally debated or decided. Only at the Butlerian level can there be reasons, below that there are merely causes. Nor is there a danger that by packing too much presuppositional meaning into an attitudinal name, we shall end by giving unwarranted sanction to some narrow canon. The ideals of impartiality and disinterestedness are the exact antithesis of everything that is narrow, and they are also so happily vague, and so capable of diverse interpretation, as to leave room for an indefinite amount of argument and adjustment even on a Butlerian plane.

I think, further, that it would be possible to defend the traditional policy of applying one and the same name to certain stringently Butlerian attitudes, and also, in a qualified sense, to attitudes that are only approximately like them. This is because Butlerian attitudes can be said to exist 'in germ' even in our most ordinary daily commerce with objects, as well as in every conversation, or in every practical dealing with our fellows. In all such intercourse we must, in a sense, put ourselves into a position which is neutral as between ourselves and others, and from such an elementary grade of impersonality we can be pushed, along natural grooves of persuadability and analogy, into almost any pitch of Butlerian stringency. It is not, therefore, nonsense to say that our attitudes always are implicitly Butlerian, since there is this natural slide or slope which leads us on, even from the most passing and personal of attitudes, to the most strenuously and stringently Butlerian ones. We reason with Nietzsche and the Mau Mau, and we condemn their policies from a Butlerian standpoint, precisely because their attitudes are ready to pass over, by slow stages and from many

small beginnings, into fully-fledged Butlerian ones. And if we ignore the important natural slide in question, we may be led, quite monstrously, to attribute the prestige of Butlerian attitudes to the contingent instruction of our parents, or to the fortuitous example of such 'innovators' as Buddha or Jesus. Whereas the roots of Butlerian attitudes lie much deeper: they could not be absent from a practical or social being. It will be well, however, to make plain, whenever we use an attitudinal name, in precisely what sense we are speaking, since Butlerian attitudes have no need of verbal subterfuges in order to gain a hold upon reflective and socially minded persons.

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## II.—SELF-REFERENCE AND MEANING IN ORDINARY LANGUAGE

BY KARL R. POPPER

- Theaetetus.* Now listen to me attentively, Socrates, for what I shall put before you is not a little tricky.
- Socrates.* I promise to do my best, Theaetetus, as long as you spare me the details of your achievements in the theory of numbers, and speak in a language which I, an ordinary man, can understand.
- Th.* The very next question which I am going to ask you is an extraordinary one, although expressed in perfectly ordinary language.
- S.* There is no need to warn me : I am all ears.
- Th.* What did I say between your last two interruptions, Socrates ?
- S.* You said : "The very next question which I am going to ask you is an extraordinary one, although expressed in perfectly ordinary language."
- Th.* And did you understand what I was saying ?
- S.* I did, of course. Your warning referred to a question which you intended to ask me.
- Th.* And what was this question of mine to which my warning referred ? Can you repeat it ?
- S.* Your question ? Let me see . . . Oh, yes, your question was : "What did I say between your last two interruptions, Socrates ?"
- Th.* I see you have kept your promise, Socrates : you did attend to what I was saying. But did you understand this question of mine which you have just quoted ?
- S.* I think I can prove that I understood your question at once. For did I not reply correctly when you first put it to me ?
- Th.* That is so. But do you agree that it was an extraordinary question ?
- S.* No. Admittedly, it was not very politely put, Theaetetus, but this, I am afraid, is nothing out of the ordinary. No, I can't see anything extraordinary in it.
- Th.* I am sorry if I was rude, Socrates ; believe me, I only wanted to be brief, which was of some importance at

that stage of our discussion. But I find it interesting that you think my question to be an ordinary one (apart from its rudeness); for some philosophers might say that it is an impossible question—at any rate one which it is impossible to understand properly, since it can have no meaning.

S. Why should your question have no meaning?

Th. Because indirectly it referred to itself.

S. I do not see this. As far as I can see, your question only referred to the warning you gave me, just before you asked it.

Th. And what did my warning refer to?

S. Now I see what you mean. Your warning referred to your question, and your question to your warning.

Th. But you say that you understood both, my warning and my question?

S. I had no trouble at all in understanding what you said.

Th. This seems to prove that two things a person says may be perfectly meaningful in spite of the fact that they are indirectly self-referring—that the first refers to the second and the second to the first.

S. It does seem to prove it.

Th. And don't you think that this is extraordinary?

S. To me it does not appear extraordinary. It seems obvious. I do not see why you should bother to draw my attention to such a truism.

Th. Because it has been denied, at least implicitly, by many philosophers.

S. Has it? You surprise me.

Th. I mean the philosophers who say that a paradox such as the *Liar* (the Megaric version of the *Epimenides*) cannot arise because a properly constructed statement cannot refer to itself.

S. I know the *Epimenides* and the *Liar* (who says "What I am now saying is untrue"); and the solution just mentioned by you does not appear unreasonable to me.

Th. But it does not solve the paradox, if you admit, as you did, that indirect self-reference is admissible. For, as Langford has shown, the *Liar* or the *Epimenides* can be formulated by using indirect self-reference instead of direct self-reference.

S. Please give me this formulation at once.

Th. The next assertion I am going to make is a true one.

S. Don't you always speak the truth?

- Th.* The last assertion I made was untrue.
- S.* So you wish to withdraw it? All right, you may begin again.
- Th.* You don't seem to realize what my two assertions taken together amounted to.
- S.* Oh, now I see the implications of what you were saying. You are quite right. It is old *Epimenides* all over again.
- Th.* I have used indirect self-reference instead of direct self-reference; this is the whole difference. And this example establishes, I believe, that such paradoxes as the *Epimenides* cannot be solved by dwelling on the impossibility of self-referring assertions. For even if direct self-reference were impossible, or meaningless, indirect self-reference is certainly quite a common thing. I may, for example, make the following comment: I am confidently looking forward to a clever and appropriate remark from you, Socrates.
- S.* This expression of your confidence, Theaetetus, is highly flattering to me.
- Th.* This proves that it may easily occur that a comment may be a comment upon another one, and that this other comment may be a comment upon the first. But once we see that we cannot solve the paradoxes in this way, we shall also see that even direct self-reference may be perfectly in order. In fact, many examples of non-paradoxical although directly self-referring assertions have been known for a long time; both of self-referring statements of a more or less empirical character and of self-referring statements whose truth or falsity can be established by logical reasoning.
- S.* Could you produce an example of a self-referring assertion which is empirically true?
- Th.*
- S.* I could not hear what you were saying, Theaetetus. Please repeat it a little louder. My hearing is no longer what it used to be.
- Th.* I said: "I am now speaking so softly that dear old Socrates cannot make out what I am saying."
- S.* I like this example; and I cannot deny that, when you were speaking so softly, you were speaking truthfully. Nor can I deny the empirical character of this truth; for had my ears been younger, it would have turned out an untruth.
- Th.* The truth of my next assertion will be even logically

demonstrable, for example by a *reductio ad absurdum*, a method most beloved of Euclid the Geometrician.

- S. I do not know him ; you don't mean the man from Megara, I presume. But I think I know what you mean by a *reductio*. Will you now state your theorem ?

Th. What I am now saying is meaningful.

- S. May I attempt to prove your theorem myself ? I assume, for the purpose of the *reductio*, the truth of the negation of your theorem, that is, the truth of the assertion : " What I am now saying is meaningless." If this assertion is true, it must be, clearly, meaningful. Thus the assumption that it is true is absurd ; which proves your theorem.

Th. You have got it, Socrates. You have proved my theorem, as you insist on calling it. But some philosophers may not believe you. They will say that my utterance (or perhaps the one you disproved, that is " What I am now saying is meaningless ") was paradoxical, and that, since it is paradoxical, you can "prove" whatever you like about it—its truth as well as its falsity.

- S. I have shown that the assumption of the truth of the assertion " What I am now saying is meaningless " leads to an absurdity. Let them show, by a similar argument, that the assumption of its falsity (or of the truth of your theorem) leads to an absurdity also. When they succeed in this, then they may claim its paradoxical character or, if you like, its meaninglessness, and the meaninglessness of your theorem also.

Th. I agree, Socrates ; moreover, I am perfectly satisfied that they will not succeed—at least as long as by " a meaningless utterance " they mean something like an expression which is formulated in a manner which violates the rules of grammar, or in other words, a badly constructed expression.

- S. I am glad that you feel so sure, Theaetetus ; but are you not just a little too sure of our case ?

Th. If you don't mind, I'll postpone the answer to this question for a minute or two. My reason is that I should like first to draw your attention to the fact that even if somebody did show that my theorem, or perhaps its negation, was paradoxical, he would not have thereby succeeded in showing that it is to be described as " meaningless ", in the best and most appropriate sense of the word. For in order to succeed he would have to show that, if we assume the truth of my theorem (or the falsity of its

negation, "What I am now saying is meaningless"), an absurdity follows. But I should be inclined to argue that such a derivation cannot be attempted by anybody who does not understand the meaning of my theorem (or of its negation). And I should be inclined to argue that, if the meaning of an utterance can be understood, then the utterance *has* a meaning; and again, that, if it has any implications (that is to say, if anything follows from it), it also must have a meaning. This view, at least, seems to be in accordance with ordinary usage, don't you think so?

*S.* I do.

*Th.* Of course, I do not wish to say that there may not be other ways of using the word "meaningful"; for example, some of my fellow-mathematicians have suggested that we call an assertion "meaningful" only if there is a method by which it can either be proved or disproved. But this would have the consequence that we could not know of a conjecture such as Goldbach's—"every even number is the sum of two primes"—whether it is at all meaningful, before we have found out whether there is a method by which it is demonstrable or refutable; for it may be neither, and no such method may exist.

*S.* I think this would be both a strange way and an awkward way of using the word "meaningful".

*Th.* And yet, other people have suggested calling an assertion "meaningful" only if we know how to find out whether it is true or false; a suggestion which amounts more or less to the same.

*S.* It looks to me very similar to your earlier suggestion.

*Th.* If, however, we mean, by "a meaningful assertion or question" something like an expression which is understandable for anybody knowing the language, because it is formed in accordance with the grammatical rules for the formation of statements or questions in that language, then, I believe, we can give a correct answer to my next question which again will be a self-referring one.

*S.* Let me see whether I can answer it.

*Th.* Is the question I am now asking you meaningful or meaningless?

*S.* It is meaningful, and demonstrably so. For assume my answer to be false and the answer "It is meaningless" to be true. Then a true answer to your question can be given. But a question to which an answer can be given

(and, moreover, a true answer) must be meaningful. Therefore, your question was meaningful, *quod erat demonstrandum*.

- Th. I wonder where you picked up all this Latin, Socrates. Still, I can find no flaw in your demonstration; it is, after all, only a version of your proof of what you call my theorem.
- S. I think you have disposed of the suggestion that self-referring assertions are impossible, or meaningless. But I am sad at having to make this admission, for it was such a straightforward way to get rid of the paradoxes.
- Th. There is a simpler way, Socrates.
- S. What is it?
- Th. Just avoid them, as nearly everybody does, and don't worry about them.
- S. But is this sufficient? Is this safe?
- Th. For ordinary language and for ordinary purposes this is both sufficient and safe. At any rate, you can do nothing else in ordinary language, since paradoxes can be constructed in it, and since they are understandable, as we have seen.
- S. But could we not legislate, say, that any kind of self-reference, whether direct or indirect, should be avoided, and thereby purify our language from paradoxes?
- Th. We can do such a thing, of course, but a language for which we legislate in this way is no longer our ordinary language; artificial rules make an artificial language. Has not our discussion shown that at least indirect self-reference is quite an ordinary thing?
- S. But for, say, mathematics, a somewhat artificial language would be appropriate, would it not?
- Th. It would; and for the construction of a language with artificial rules which, if it is properly done, might be called a "formalised language", we shall take hints from the fact that paradoxes (which we wish to avoid) can occur in ordinary language.
- S. And you would legislate for your formalised language, I suppose, that all self-reference must be strictly excluded, would you not?
- Th. No. We can avoid paradoxes without using such drastic measures.
- S. Do you call them drastic?
- Th. They are drastic because they would exclude some very interesting uses of self-reference, especially Goedel's method of constructing self-referring statements, a method

which has most important applications in my own field of interest, the theory of numbers. They are drastic, moreover, because we have learned from Tarski that in any consistent language—let us call it “L”—the expressions “true in L” and “false in L” cannot occur, and that without expressions such as these, paradoxes such as the *Epimenides*, or Grelling’s paradox of the heterological adjectives, cannot be formulated. This hint turns out to be sufficient for the construction of formalised languages in which these paradoxes are avoided.

- S. Who are all these mathematicians? Theodorus never mentioned their names.
- Th. Barbarians, Socrates. But they are very able. Goedel’s so-called “method of arithmetisation”, more especially, is interesting, from the point of view of our present discussion.
- S. Another self-reference, and a very ordinary one. I am getting a little too conscious of these things.
- Th. Goedel’s method is, one might say, to translate certain non-arithmetical assertions into arithmetical ones; they are turned into an arithmetical code, as it were; and among the assertions which can be so coded there happens to be also the one which you have jokingly described as my theorem. To be a little more exact, the assertion which can be turned into Goedel’s arithmetical code is the self-referring statement “This expression is a well-formed formula”; here “well-formed formula” replaces, of course, the word “meaningful”. I felt, you will remember, a little too sure for your liking that my theorem cannot be disproved. My reason was, simply, that when turned into the Goedelian code, my theorem becomes a theorem of arithmetic. It is demonstrable, and its negation is refutable. Now if anybody were to succeed, by a valid argument (perhaps by one similar to your own proof) in disproving my theorem—for example, by deriving an absurdity from the assumption that the negation of my theorem is false—then this argument could be used to show the same of the corresponding arithmetical theorem; and since this would at once provide us with a method of proving “ $0 = 1$ ”, I feel that I have good reasons for believing that my theorem cannot be disproved.
- S. Could you explain Goedel’s method of coding without getting involved in technicalities?

- Th.* There is no need to do this since it has been done before—I do not mean, before now, the supposed dramatic date of this little dialogue of ours (which is about 400 B.C.) but I mean, before our dialogue will ever be concocted by its author, which won't take place before another 2350 years have elapsed.
- S.* I am shocked, Theaetetus, by these latest self-references of yours. You talk as if we were actors reciting the lines of a play. This is a trick which, I am afraid, some playwrights think witty, but hardly their victims; anyway, I don't. But worse still than any such self-referring joke is this preposterous, nay, this meaningless chronology of yours. Seriously, I must draw a line somewhere, Theaetetus, and I am drawing it here.
- Th.* Come, Socrates, who cares about chronology? Ideas are timeless.
- S.* Beware of metaphysics, Theaetetus!

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### III.—WHAT THE TORTOISE TAUGHT US

D. G. BROWN

LEWIS CARROLL himself sided with Alice, and omitted to give the moral of the story of Achilles and the Tortoise (MIND, 1895, pp. 278-280). The Duchess and more recent logicians have rather been willing to draw many morals than successful in making any one of them obvious. What, after all, is the moral of the story?

1. I think there are two morals. The first is this: the legitimacy of inferring that since  $p$ ,  $q$ , does not require the truth of the statement that if  $p$ , then  $q$  in the same way as it requires the truth of the statement that  $p$ .

This version of the first moral needs to be explained; and the explanation will serve also to show what truth there is in the common version, to the effect that one must not treat the principle of an inference as a premiss.

1.1. No doubt the way in which ' $p$ ' is required for the legitimacy of the inference to ' $q$ ', and the way in which 'If  $p$  then  $q$ ' is required, differ in many respects. I shall first distinguish the relevant respect in which they differ, and then say how disregard of this difference generates the regress.

The legitimacy of the inference to ' $q$ ' requires both the truth of the premiss, ' $p$ ', and the validity of the inference. The statement whose truth is here said to be required is the premiss. The validity of the inference, in its turn, requires (and is required by) the truth of 'If  $p$ , then  $q$ ', and thus the legitimacy of the inference to ' $q$ ' requires as well the truth of 'If  $p$ , then  $q$ '. The statement whose truth the legitimacy of the inference is now said to require is not the premiss, but the appropriate specification of the principle of the inference.

Now, the way in which the truth of 'If  $p$ , then  $q$ ' is required is different from the way in which the truth of ' $p$ ' is required. For the truth of the premiss leaves it open whether the inference is valid; the validity of the inference leaves it open whether the premiss is true; both are required for the legitimacy of the inference. But the truth of the premiss is the truth of the premiss, while the validity of the inference is the truth of the hypothetical. It follows that to require the truth of ' $p$ ', before accepting the legitimacy of the inference to ' $q$ ', is to require that which, when provided, leaves it open whether the inference is valid. On the other hand, to require the truth of 'If  $p$ , then

q' is to require that which, when provided, settles the validity of the inference. It is in this sense that the truth of 'If p, then q' is required in a different way from that of 'p'.

Suppose now that this difference is ignored. Suppose that having accepted the truth of 'p' we require the truth of 'If p, then q' in the way in which we require the truth of 'p', before accepting the legitimacy of the inference to 'q'. This is to require of 'If p, then q' that which when provided leaves it open whether the inference to 'q' is valid, just as normally the truth of 'p' leaves it open whether the inference is valid. It is to do this, precisely where we had set out to require that which, along with the truth of the premiss, settles at the same time the validity and the legitimacy of the inference. The result is that 'p, and if p, then q' becomes the conjunctive premiss, in just this sense that having required and been provided with the truth of this conjunctive statement we may still raise the question of the validity of the inference to 'q'. But then, according to the requirements we are supposing ourselves to make, the legitimacy of the inference to 'q' requires, through requiring the validity of this new inference, the truth of a new hypothetical, and requires it in the way in which it requires the truth of 'p'. To require its truth in this way is by now recognisably to take the second step along a vicious regress.

1.11. Such an interpretation of the story has been given briefly by Professor Ryle ('If, so, and because', *Philosophical Analysis*, ed. Black, p. 328). There he gives a positive account, in terms of the notion of 'application', of the way in which the legitimacy of the inference to 'q' does require the truth of 'If p, then q'. I have said nothing about that. I have kept to a minimum statement of the negative point which the story yields.

1.2. When, instead of saying all this, we merely say that one must not treat the principle of an inference as a premiss, we leave the matter fuzzy in three places:

1.21. The phrase 'the principle of an inference' is an expression to which we have not been properly introduced, and whose appearance in formal discussion causes embarrassment. Joseph has acquainted us with principles upon which various kinds of inference proceed (cf. Ch. XIV of his *Introduction to Logic*, called 'Of the principles of syllogistic inference'). He speaks of Euclid's first axiom as 'the principle in accordance with which we reason' in a schema of argument which he gives. Again, the so-called 'principle of inference' to be met with in symbolic logic, though it is sometimes not a principle of inference, but a rule of a calculus, may in some books be so defined as to be such

a principle of one sort of inference : the sort in which the conclusion that  $q$  is drawn from the fact that  $p$  and that if  $p$  then  $q$ . Then when one takes as an example of the kind of inference an actual inference in accordance with the principle, it seems natural to speak of the principle as being 'the principle of this particular inference'. No difficulty arises about the uniqueness of the principle. But suppose that one begins from a particular inference, e.g. the inference, from the fact that today is Monday, that tomorrow is Tuesday. It is not at all clear what *the* principle is. Is it that if any day is Monday, the next day is Tuesday? Or is it that if any day on which one is speaking is Monday, the day after the day on which one is speaking is Tuesday? Or is it that if any day is a named day of the week, the next day has the name next in the recognised circular list?

Of course there is one hypothetical which is unique, and which stands in the same relation to each of these principles, namely that if today is Monday, tomorrow is Tuesday. But clearly we shall demand of a principle that more than one inference can be in accordance with it; this hypothetical is not a principle at all. Where any principle would say 'any', this statement says 'this one'. It is completely 'meaty', 'closed', 'determinate', or as I shall say, 'specified'. Its relation to each of the principles is that of being the (complete) specification of it.

Evidently the principle of the inference that since  $p$ ,  $q$  would have to be one among the many hypothetical statements, any one of which has the statement that if  $p$ , then  $q$  as a specification. Some logicians do explicitly claim that every inference involves the thought of one particular principle (cf. Bradley, *Principles of Logic*, pp. 522-524 or Peirce, *Collected Papers*, 1.462), but no defence of this rash claim is forthcoming.

It is interesting for other purposes to ask how there comes, when there does, to be one among these statements which is *the* principle. A short answer is that one determines the principle in one's choice of ground on which to defend or attack the validity of the inference. To ask 'Is it so that if  $p$  then  $q$ ?' is to raise the issue of its validity; to go on to a principle is to examine the issue. The long answer need not be given here. For the legitimacy of the inference requires the truth of one or more of these principles in the same way as it does that of the specification. Thus the moral could be generalized so as to refer to the way in which the truth is required not merely of the specification, as in the actual story, or of the principle, where there is a single one, but of any required one among this range of hypotheticals.

1.22. To identify an inference as the inference from the premiss 'p' to the conclusion 'q', and to define the principle of it as a statement having 'If p, then q' as a specification, is to make it flatly contradictory to suggest that the principle of the inference is a premiss. How anyone could fall into so flat a contradiction would be mysterious. It is accordingly unsatisfactory to say that the regress arises from making simply this mistake.

But inferences can be identified in a different way. For it is a suggestion which always needs to be considered that the premiss 'p', though true, is insufficient to establish the truth of 'q'. The suggestion normally amounts to this, that if the inference to 'q' is to be valid, there must be provided some other premiss besides 'p', and the inference must be some other inference than that from 'p' to 'q'. The peculiarity of the present argument is merely that it is the very statement, 'If p, then q', which is said to be needed, as an additional premiss conjoined with 'p', in order to establish 'q'. The entrance to the regress stands open only when we identify the inference by its conclusion; for the entrance is the question of what, along with 'p', the premisses must comprise for the inference to 'q' to be valid, and this question depends on so identifying the inference. We step through this entrance into the regress only when we answer that 'If p, then q' is in this sense required, *i.e.* when we treat 'If p, then q' as a premiss.

1.23. We have been brought to the third obscurity. What is it to leave open what the premisses are and what is it to go on and treat 'If p, then q' as though it were a premiss? For evidently we may, if we have the whim, construct new arguments with redundant premisses. Though it is futile, it is not impossible in this sense to treat 'If p, then q' as a premiss. What we must not do is rather to *require* that 'If p, then q' be regarded as a premiss before admitting the legitimacy of the inference to 'q', in so far as this is to require the truth of 'If p, then q' in the way that we require the truth of 'p'. We may, of course, require the truth of the hypothetical in the way that we still require the validity of any inference even after enough true premisses are on hand. But when, setting out to do this, we require it in the other way, we re-open, in defiance of our hypothesis, the question which is the door to the regress, namely 'What premisses are required?' Since at the same time we answer that 'If p, then q' is the premiss required, we at once walk through it into the regress.

The regress is generated therefore by 'treating the principle as a premiss' in a sense which wants careful elucidation. To

sum up roughly, the trick consists in this, that one begins from 'the inference to "q"', considers what is required for the legitimacy of this inference, and then in a certain order alternates between two ways of holding the truth of statements to be required for the legitimacy of the inference.

1.3. It might be objected that unless 'If p, then q' is analytic, the inference to 'q' is indeed not valid, so that the legitimacy of the inference does in fact require the truth of 'If p, then q' as a premiss. In other words, the premiss must be 'p, and if p then q', and the hypothetical which is the specification of the principle of the inference must be 'If it is so that p, and that if p then q, then q', which is analytic. In such a case, the claim is, the vicious regress would be entered upon only at the second stage, when the truth of this second hypothetical, or of the principle of 'modus ponens' of which it is a specification, is required in the way one requires the truth of a premiss. (Cf. D. F. Pears, *MIND*, 1951, p. 554, reviewing Ryle.)

The issue raised by this objection is a verbal one. It is a verbal issue in which nothing is at stake for my point, and in which a great deal is at stake from other points of view. The objection amounts to the insistence that 'valid' shall be taken to mean 'deductively valid', i.e. 'such that the conclusion "follows logically" from the premisses'. For if this is what 'valid' is to mean, the objection is well taken. But it is equally clear that if 'valid' is to mean 'such that from the premisses the conclusion can properly be drawn, in view either of certain relations of entailment, or of certain general empirical facts, or of something else', then my account stands quite generally. For the purposes of this paper, a reader who insists on the narrower use of 'valid' may simply agree with the objector, and then restate my general point by introducing some technical term of his own with the same use as I have given to 'valid'.

For other purposes I should strongly defend my non-ordinary use of the technical term 'valid', in which its application is as wide as the ordinary application of the non-technical words 'legitimate', 'unwarranted', 'well-grounded', 'unsound', 'conclusive', 'does not follow', 'correct to conclude', 'need not imply' and 'is enough to show'. But it would be necessary to examine the many similarities and differences that are saved or lost in both ways of speaking. Here I shall point out only that a larger verbal issue stands behind this one. For it would normally be insisted that 'validity' be precisely what, along with true premisses, an inference should have, and what anything purporting to be a legitimate inference was laying claim to.

It would be an odd sorting out of terms which allowed that a legitimate 'deductive' inference had to be valid, but a legitimate inference of some other kind could be not valid. There are analogous relations between 'valid' and the other non-technical words mentioned. Thus the restriction on the use of 'valid' naturally goes with the restriction of inference proper to 'deductive' inference.

2. The second moral, shorn of qualifications, is this: A man knows that if  $p$ , then  $q$  if, when he knows that  $p$ , he is able to see that, consequently,  $q$ . If a man knows that  $p$ , but cannot see that  $q$ , this is just what shows him not to know that if  $p$ , then  $q$ . In other words, knowledge of the hypothetical 'If  $p$ , then  $q$ ' is such that there cannot be a man of a kind the Tortoise said might exist, namely his second kind of reader who accepted ' $p$ ' and 'If  $p$ , then  $q$ ' but failed to see ' $q$ '. The qualifications needed will come out as this point is sharpened. This can be done with the help of a treatment of irrationality which I have learned from Mr. Peter Herbst.

2.1. Consider what must be said of a man who says 'The Russians will attack in Europe this autumn', and says 'If the Russians attack in Europe, there will at once be bombing of this country', and who nevertheless also says, 'I am not at all convinced that there will be bombing of this country in the autumn'. Of course there could be such a man; and not merely because some people will say anything, and most people will on occasions change their minds. It might be clear both that our leg was not being pulled and that all three remarks were being offered concurrently.

Certainly his remarks cannot be taken at their face value. To find out what he is getting at we could draw his attention to the inconsistency in what he says. Perhaps he will be found to use 'at once' for 'at one time or another', his broken accent giving the explanation, or to have intended his third remark to express, paradoxically, his revulsion at the idea that the country would be bombed. At any rate, the presumption is that through ignorance, mistake, or eccentricity he is not making a standard use of his words.

However, after a moment's argument he may on the other hand be shocked to discover the implications of his first two remarks, and at once become convinced that the country will be bombed. Though hardly likely in such a simple case as this, it is possible even here that a man should just hold inconsistent views without realizing it. Where the inconsequence is less explicit, and must be elicited by an extended argument, we are

all frequently in that position. Here, what the man needs is to have the argument brought before him. Though he did not at first see that it followed that there will be bombing of this country, after possibly protracted but not unlimited consideration, he does see. He is able to see it.

But suppose argument does no good ; suppose he is not able to see it. It may even now emerge that after all we have on our hands a person somehow radically defective. However, such cases must be set aside, for, as applied to them, the usual notions of what someone thinks and how he thinks begin to lose their grip. The man may just not be up to the level of such ideas, and though he has a curious facility in chatting on, not understand a word he is saying ; or he may be an advanced psychotic. But whether he lies in the direction of those with brain injuries, and of the animals, who are imperfectly rational, or in that of madmen who are completely irrational, it will be indeterminate what views he holds, or else clear that he holds none.

Are there, finally, milder cases, in which a man can be said to hold the views expressed in the first two remarks, and yet is unable to see their implications even after reasonable consideration ? There is one such case, and thus one solid qualification to be attached to my statement of the moral. For it is possible to be irrational about a matter, in the sense that one can have a localized irrationality—a blind spot, an emotional blockage, a ' thing ' about such things. It is, however, correct to assume such a local irrationality only in someone who has been shown independently (a) to be using his words intelligibly, (b) to hold the views he professes, (c) to be clever enough to see the point, and (d) to be on the whole a rational person.

To sum up, it is only in the case of a local irrationality that a man can be said to know that *p*, and that if *p* then *q*, and yet be unable to see that *q*. For unless it is such a case, the fact that he does not after argument say ' *q* ' will show him to be insincere, or to speak eccentrically, or not to understand the statements that *p*, or that if *p* then *q*, or not really to think that *p*, or, in the most straightforward case, not really to think that if *p*, then *q*.

2.2. It might be thought that mild stupidity gave rise to all-important exceptions, since it is possible that someone should understand the statement that *p* and the statement that *q*, while the more complex statement that if *p* then *q* exceeded his grasp. However, it is clear that nothing follows from this possibility. It is in fact the possibility all over again that some-

one should know that *p*, and understand the statement that *q*, and yet fail to see that *q*. But it is still absurd to suppose that someone could see that *p*, and that if *p*, then *q*, and be unable through stupidity to put these together and so arrive at *q*. For if, knowing that *p*, he is too stupid to see that *q*, he is too stupid to see that if *p*, then *q*. If he does not understand that *q*, there is nothing left for him to understand by the statement that if *p*, then *q*. On the other hand, if the fact that *p* convinces him that *q*, then he thinks that if *p*, then *q* even though he never formulates it.

2.3. Of course, to this intimate relation between knowledge of hypotheticals and ability to see conclusions there must be added, in the case of analytic hypotheticals, an intimate relation between knowledge of hypotheticals and ability to use the words in which they are expressed. The situation is thereby complicated for such hypotheticals as that if today is Monday, tomorrow is Tuesday, and the Tortoise's that if *A* and *B* be true, *Z* must be true. Anyone who could not, in appropriate circumstances, see that tomorrow is Tuesday, or that *Z* must be true, would not know these hypotheticals to be true. But it would follow that, irrationality apart, he did not understand them either. Where a sentence 'If *p*, then *q*' has been correctly used to express an analytic statement, anyone who cannot see the statement then made to be true does not know the use of 'if . . . then . . .', or '*p*', or '*q*', or all three. This point can be made without explicitly bringing in the hypothetical. Thus anyone who in such a case uses '*p*' and 'It is not so that *q*' to make statements, meaning what he says, does not know the use of both '*p*' and 'It is not so that *q*'.

2.4. Professor Ryle, who has suggested the first moral for the story, also hinted some years before that the very opposite of this second moral could be drawn from it. In 'Knowing how and knowing that' (*P.A.S.*, 1945-6, p. 6), he implies that 'Lewis Carroll's puzzle' arises from the possibility of there being, say, a pupil who understands the premisses and conclusion of an argument, and on being told that if these premisses are true, the conclusion is true, 'understands this and dutifully recites it alongside the premisses, and still fails to see that the conclusion follows from the premisses even when accompanied by the assertion that these premisses entail this conclusion'. This, it seems, conflicts with the received view that 'knowing how to reason . . . [is] . . . analysable into the knowledge or supposal of some propositions . . .'. So the point of the story is that there can be such a person, and that certain views of reasoning

are therefore precluded. I take it Ryle assumed both that seeing a conclusion to follow is a case of reasoning, and that knowing how to reason is, in his sense, a case of 'knowing how'. I take it also that in speaking of 'the assertion that these premisses entail this conclusion' he had in mind rather the necessarily true hypothetical statement that if these premisses are true, this conclusion is true.

But none of this will do. The case described is not possible. There is no puzzle provoked by its possibility, and no call here for the distinction between 'knowing that' and 'knowing how'. When it is a question of stupidity, inability to see the conclusion is sufficient to show that the pupil does not understand and accept the hypothetical, regardless of what he 'dutifully recites'. It is doubly so, in this case, since the premisses entail the conclusion. But it would be so even if the inference had to be justified, i.e. the truth of the hypothetical established, by empirical investigation.

The phrase, 'dutifully recites', together with 'learn the formula by heart' and 'the recitation of formulae', betrays a partial recognition of this fact, for it confesses that the state of the pupil who says the words must be one in which the other criteria for testing his understanding and acceptance are not satisfied. However, this distinction between imperfect and full, merely verbal and actual, or superficial and genuine, Ryle is also inclined to represent as a distinction between theory and practice. But the accounts are incompatible. One must choose between saying that something is not fully understood, and saying that it is understood, even if theoretically; between saying that something is only verbally accepted, and saying that it is accepted, whether the acceptance be theoretical or practical. He also refers to differences between acknowledgment and contemplation on the one hand, and application and performance on the other. But the relevant antithesis to 'propositional acknowledgment' would here be 'really believing', and to 'mere contemplation', 'catching on'.

I think it can be shown to be no accident that the notions of practice, application, and performance, as roughly opposed to theory, are irrelevant. For 'seeing that a conclusion follows from premisses' is not practice. When I conclude that something is so, there is no application by me of anything. Though understanding the hypothetical entails a capacity, it is not one exercised in performances. But all three stories are too long to tell here.

Three things nevertheless need to be said, so as not to over-

simplify the connexion between these claims and the above criticism of Ryle. First, I imply nothing about Ryle's account of 'knowing how' as applied to genuine activities like playing chess and cooking. Second, on the previous page (p. 5), Ryle does consider and reject the antithesis between 'a set of verbal habits' and 'really' or 'fully' knowing. But curiously he rejects it at a point where he ought on his own views to embrace it. He implies that his chess player does really 'know these truths', but is too stupid to apply them. But 'these truths' are actually the 'rules, tactical maxims, "wrinkles", etc.' which have been imparted to him; and on page 7 we learn that 'Knowing a rule is knowing how. It is realized in performances which conform to the rule, not in theoretical citations of it.' Thus 'rules' have got substituted for 'truths', and though we started out to discuss truths that are known without the knowledge of them being any help, we have all along been talking about rules that are not really known. Third, I do not, on the other hand, deny that there are truths (*e.g.* expressed with 'ought') of which, as of rules, the knowledge is 'realised in performances'. But I do deny that statements of which 'If p, then q' can be a specification are truths of this kind and, if such statements are 'rules of inference', that 'rules of inference' are performance rules.

3. It is because of the peculiar relation between a hypothetical statement and certain arguments and inferences that hypothetical statements (1) are not parts of arguments, as premisses are, and (2) cannot be understood by those who could not understand certain arguments. So the moral of both morals is that the meaning of a hypothetical statement is to be elucidated in terms of its relation to arguments and inferences.

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#### IV.—SENSE DATA: A SUGGESTED SOURCE OF THE FALLACY

BY J. R. JONES

It has never been clear to me whether one was supposed to be *aware* of one's 'sense data'. Suppose I am seeing a teacup. According to a certain theory this means that I am also, in some manner, apprised of the presence of a sense datum. If, however, I protest that I am quite sure that a teacup is all I can see, I am told that I must understand that my being aware of the sense datum is so oddly related to my being aware of the teacup that I, the owner of both awarenesses, am ordinarily unaware of the difference between their respective objects. But surely this is to admit that I am *not* aware of the sense datum. The usual retort is that it is to admit only that I am not *explicitly* aware of it. For, it is claimed, not to notice the difference between a teacup and another object *x* is to be *implicitly* aware of this other object. But how can I *know* that I am implicitly aware of an object *x* if the implicitness of this awareness is only another name for my unawareness of any object distinguishable from the teacup that I am perceiving? And what is your proof then that *x* exists? The Sense Datum Theory turns out to be phenomenologically groundless.

Its supporters, however, refuse to capitulate. "However hard you look you say that you cannot bring anything besides the teacup into view. This is because sense data are ordinarily so concealed in the physical objects which people think they are perceiving that direct inspection alone is powerless to uncover them. But there is another operation such that if you were to perform *this* operation on your seeing the teacup the report of your awareness as then revealed by direct inspection would be that you are actually only seeing an entity of the sort we mean by a sense datum. This operation we call *perceptual reduction*". Now I doubt whether its critics fully appreciate that the Sense Datum Theory is thus making its final stand on a claim to the possession of an indirect means of access to sense data. Surely it is on this point that the final reckoning must come. And it will not do simply to deny that perceptual reduction is possible. We have to show what it is that philosophers are doing when they think that they are 'reducing' a percept and exposing an ingredient which they believe to be

otherwise concealed in it. That is what I am proposing to attempt in this paper.

In the first instalment of his important article on *Sense Data and the Percept Theory*,<sup>1</sup> Mr. Roderick Firth expresses the suspicion that perceptual reduction is merely a method of "substituting one state of awareness for another". And in one or two places he significantly slips into describing it as an *introspective* operation. What made this description seem appropriate? My own view is that when Mr. Firth attempted a perceptual reduction, being a close observer of his mental processes he discovered that at the crucial stage in the operation—the stage at which a concealed ingredient of the scene before him should be coming into view—he was not really attending to the percept itself but was engaged in some inwardly directed activity of a substitutive nature. I submit that this was because the operation is not transacted in the actual world at all but is an imaginary transaction involving the substitution of a fictitious object for the object which we ordinarily take ourselves to be perceiving. To substantiate this view I shall have to do two things. First, I must show where I think this fictitious object came from. And then I must explain how a fiction could be so mistaken for a genuine entity that its substitution in imagination for the object we ordinarily perceive could masquerade as the genuine exposure of a concealed ingredient of this object.

Perception I take to be a way of being aware. And I would distinguish three parts to the structure of the situations describable as awarenesses. There is first the 'being aware'. This is dismissed in certain quarters today as unwitnessable and, therefore, ghostly. Publicly unwitnessable it certainly is, but only wraithlike if presumed to be a quasi-substance. I make no such presumption. If I am seeing a teacup something is happening which I call my being aware. There is no mystery about this. I just am aware of what I am seeing. But so far from being a ghostly process that is going on inside me, this being aware reduces phenomenologically to a fact about the teacup. This fact is not strictly describable. But it may be indicated metaphorically by naming it the fact that the teacup 'appears' or 'comes to light' or is 'presented' or 'revealed'. I would distinguish next in the structure of an awareness the object which thus appears or comes to light. It is a main contention of this paper that in ordinary perceptual situations objects of one sort only appear or come to light, namely, such objects as tables and teacups. The third element I would

<sup>1</sup> *MIND*, LVIII, 232; pp. 434-465.

characterise as follows. The fact of being aware in itself is perfectly general. It is the elemental fact of apprisement or intimation that is common to every state of mind which possesses an object. But every time that I am aware, it is some particular object that I am aware of. And it seems to me that something sharing this particularity of the object must be discernible in the awareness of it. If a *teacup* appears or comes to light, then this particular awareness has a character or content which makes it the particular awareness that it is and distinguishes it from every other. The content which I mean I can only describe as 'the appearing or coming to light of the teacup in question'. For the purposes of this paper such concrete 'appearings' or 'comings to light' are the important elements in the structure of a perception.

Now there are as many distinct classes of appearings of physical objects as there are sense-modalities served by the specialised receptor mechanisms. Each such class, considered as having possible rather than actual members, I shall designate an 'occupyable sense-field'. Sense-fields are delimited phenomenologically by the fact that, in an indicable sense of the phrase 'belongs exclusively to', only a certain selection of the sensible qualities of an object belong exclusively to the same sense-field. The sense of the phrase in question is indicated by an example. Thus the colour of an object belongs exclusively, in the sense I mean, to the visual sense-field. I propose to use the description 'proprietary' for the characteristic of belonging exclusively (in this sense) to a given sense-field. This usage will enable me to describe, for example, colour as a sensible quality proprietary to vision. I shall also refer to the visual field itself, considered apart from any relation into which it may enter with other sense-fields, as 'the proprietary visual field'. And I shall refer to the other sense-fields, similarly abstracted, in the same way. We start then with the proposition that there coexist a number of proprietary occupyable sense-fields wherever an undamaged neurosensory organism is functioning normally.

Now I would distinguish two senses in which a sense-field becomes occupied when a physical object is perceived. If a perception occurs, one or more proprietary sense-fields will be occupied in a clearly recognizable sense by the *object* which thereby appears or comes to light. This I shall call the *presentational occupation* of a sense-field. 'Occupying a sense-field' in this sense means the same as 'coming to light' or 'appearing in consciousness'. Physical objects alone can occupy sense-fields in this way. But in any perception, besides the fact of appear-

ing in general and the given object, there is what I am calling 'the appearing of that particular object'. It seems to me that, whenever a physical object presentationally occupies a sense-field, there is another, less obvious, sense of 'occupies a sense-field' in which the *appearing* of that object occupies the sense-field in question. I propose to describe this second occupation by saying that when a proprietary sense-field is presentationally occupied by a certain physical object, that same sense-field is *internally occupied* by the appearing or coming to light of this object. Thus what internally occupies a sense-field is not the object perceived, but that object's occupying the sense-field in the way required to bring it to light.

What is the logical status of the internal occupants of sense-fields? There are no names for these entities in language unless, for example, names like 'sight', 'glimpse', 'look', etc., which Professor Ryle classifies as 'nouns of seeing', be regarded as naming the class of those appearings which are proprietary to vision. Certainly, by a 'sight' we do not mean seeing in general nor do we quite mean the object seen, but rather that particular occupation of our visual field which is some object's being seen. There is, however, another method of directing the reader's attention to the entities I mean by 'appearings of physical objects'. To obtain this concept, we have had to think of sense-fields as occupied in two distinct senses. Now there is an analogous duality in the structure of the situation expressed by certain action verbs, and this duality finds a perfectly natural expression in language. Verbs like 'strike' and 'jump' are examples. Thus when, in one sense of 'strike', I strike, say, a man, I *eo ipso*, in another sense of 'strike', strike a stroke. Similarly, in jumping a fence, I *eo ipso* jump a jump. In contrast with the man that is struck or the fence that is jumped, the *stroke* that is struck or the *jump* that is jumped are said to stand as cognate or internal accusatives to the corresponding verbs. I will not, of course, be saying anything new, but I shall say that when, for example, a physical object occupies my visual field in the ordinary sense of being something which I am seeing, my seeing this object or the object's being seen occupies my visual field in somewhat the same sense as a stroke is struck by me whenever I strike a man or a jump is jumped by me whenever I jump a fence. I shall express this by calling the proprietary appearings of physical objects the cognate or internal accusatives of perception. Now note that it *follows* from this analysis that, intrinsically, the accusatives in question are not objects. I am not *aware of* an object's appearing to me

except in somewhat the same sense as a stroke is struck by me when I strike a man. In the sense of 'aware' which corresponds to the sense of 'strike' in which I strike, not the stroke but the man, I am only aware of the physical object that is appearing to me. The point may be expressed in terms of the distinction between occupying a proprietary sense-field 'presentationally' and occupying it 'internally'. It would be as meaningless to suppose that what occupies, say, my visual field internally also occupies this field presentationally, that is, that what I am calling the object's 'appearing' is itself something seen by me, as it would be to suggest that the stroke which I strike in striking a man is struck by me *in the same sense of 'strike'* as the man is struck. 'Stroke' is a noun of striking. It names what happens when I strike a man. Similarly, such and such an object's appearing or occupying my visual field is what happens when I see the object in question. It is a fallacy to suppose that, in addition to seeing *this* object, I also *see* the happening which is my 'seeing' it, as if the object's 'being seen' were itself an object that I could look at. This fallacy plays an important part in my argument and I need a name for it. Adopting the suggestion 'alien' for the opposite of 'cognate' (accusative), I propose to call it the fallacy of *alienating an internal accusative*. It is, I suggest, the fallacy which Professor Ryle is illustrating when he points out the absurdity of making what is already a noun of eating into an accusative alien to the activity and talking of 'eating nibbles of biscuits'.

What is the connexion between saying that there occur proprietary appearances of physical objects and saying that every physical object that is perceived presents some appearance? 'Appearing', as I have been using the term, refers generically to the fact of an object's occupying some sense-field. But an object that is thus, generically, appearing is occupying some *specific* sense-field and is necessarily subject to any conditions of occupation which may obtain in that field. An object, in other words, occupies a sense-field subject to the latter's proprietary conditions of appearance. Thus, occupation of the visual field is subject to the exigencies of illumination, reflection, refraction, perspectival distortion and so forth. This introduces the important sense of 'appearing' which involves the possibility of a thing's appearing to have sensible qualities which it may not in fact have. And by an 'appearance' in the Theory of Perception is ordinarily meant a thing's appearing (in my sense of 'appearing') under conditions which may thus be making it seem different from what it is. An appearance, in

short, is an appearing considered from this particular point of view. And it therefore *follows* that perceptual appearances are not objects. It is sometimes said that if a round object which I am seeing appears elliptical, I am seeing something which *is* elliptical. Certainly, if under the given perspectival conditions, a round object is appearing as if it were elliptical, there is a perfectly natural sense in which the *appearing*, in that case, *is* elliptical. But it by no means follows that an elliptical *object* is then appearing. Deprecating the tendency to harden appearances into objects, Dawes Hicks wrote: "When in ordinary language we speak of the objective constituent of a perceptual situation as being the 'appearance' of a physical object, we mean not that it is the appearance which appears but that it is the physical object which appears."<sup>1</sup> In fact, it can no more be the appearance which appears than the stroke which I strike in striking a man can be something which I strike in the same sense that I strike the man, or the jump which I jump in jumping a fence can be something which I jump over. There are no such things as strokes or jumps hanging around waiting to be struck or jumped in the way there are men one could strike or fences one could jump hanging around. Similarly, the world does not contain, in addition to the ordinary objects that we see, certain appearances of those objects which we also see in a like sense of 'see', although appearances of objects are certainly involved in our seeing objects somewhat as strokes are involved in our striking men or jumps in our jumping fences.

Proprietary appearings of objects, then, are occupypings of a percipient's proprietary sense-fields by physical objects, involving the imposition of two sorts of limitation upon the occupying object: (1) As occupying such and such a sense-field, the object is presented as having only that selection of its sensible qualities which is proprietary to the sense-field in question. (2) The object occupies the sense-field subject to any conditions of appearance which may obtain in that particular field.

Next I would distinguish two forms in which there is reason to think such proprietary appearings can occur in the course of experience. It seems to me certain that the field in which I am ordinarily seeing things like tables and teacups is not my proprietary visual field. As a result of the process known to psychologists as 'complication', such objects appear in a composite field which is the product of a conflation of a number of proprietary fields. This involves more than that, on perceiving an object's visual qualities, one is associatively reminded of certain

of its other qualities. It means that an ordinarily seen object is not seen as having only the sensible qualities that are proprietary to vision, but presents itself to sight alone as having a fuller complement of qualities, and particularly as having tactile (or tangible) qualities. This is not to deny that when an object is seen an occupation of the proprietary visual field is then taking place. The proprietary sense-fields are retained in a coalesced form within the composite field—retained, that is, in such wise that they can no longer normally occur in isolation. The composite field I propose to call the *perceptual field* and I shall express the last point by describing the proprietary fields as *sublated components*<sup>1</sup> *within this*.

Now I think it possible to distinguish *two* forms in which the proprietary appearings occur in the course of experience because I consider it to be a fair inference that every sensory biography passes through an inchoate, pre-perceptual, phase when the proprietary sense-fields simply coexist in a form uncoalesced and unmodified by each other's deliverances. Such primitive fields would belong together implicitly as correlated to neuro-sensory happenings in the same organism, but not yet fully explicitly as centred in a sentient Subject that is conscious of unity amid the diversity of its modalities and successive states. Sensations of various sorts, however, would be occurring from the outset. There can be no time postnatally (except during anaesthesia or sleep) when simultaneous and successive occupancies of one's sense-fields are not taking place. Adult observers know that what must be occupying the infant's sense-fields are its own body and the various other physical objects in the immediate neighbourhood of this. But it would be presumptuous to think that the infant itself knows as much. All we can safely say is that physical objects are implicitly occupying its uncoalesced sense-fields. Phenomenologically the problem then becomes, What, in that case, may be inferred to be the explicit nature of the infant's sensory experience? The reconstruction I would tentatively submit is that it must consist of a mass of proprietary appearings which are *neither* understood to be physical objects appearing in the various modalities *nor* apprehended as being themselves objects of a peculiar, non-physical, sort. The fact that no 'complication' of sense-fields has yet taken place entails that the appearings will be *proprietary*. That is, the infant's visual sensations, for example, will be

<sup>1</sup> By this I mean (adapting the Hegelian usage) a component that is both effaced considered as an independent part and yet preserved within the larger whole.

'physical objects' presented as having only that part of their complement of sensible qualities which is proprietary to vision, and in a form uncoalesced with the appearances presenting the other parts of their full complement of qualities. I have put 'physical objects' in quotation marks in that sentence because it is only in an *implicit* sense that these proprietary appearances can be said to be presentations of physical objects. This is a point that I find hard to express. Unknown to itself the infant organism is subject to constant sensory stimulation by such things as rattles, milkbottles and human hands and faces. Explicitly, however, its sensations are appearances of these objects such that the objects themselves do not appear. It is a situation where comings to light are occurring but nothing actually comes to light. For, *ex hypothesi*, the *physical objects* do not come to light; otherwise the infant's perceptual experience would be indistinguishable from our own. And if we suppose that the comings to light themselves come to light, that is, occupy their respective fields presentationally as well as internally, we commit the fallacy of alienating an internal accusative. In short, our reconstruction involves the very radical consequence that at this primitive, pre-perceptual stage, experience has no *objects* at all. It has no objects except in the sense that surrounding bodies, their sensible qualities portioned out among a number of uncoalesced proprietary sense-fields, constitute its *implicit* objects. And where there are no explicit objects, it is common ground that there can be no acknowledged distinction of subject and object. There is simply a consciousness filled with implicit appearances of surrounding bodies, almost in the way a headache is a state filled with the quality describable as 'headachy feeling', and not an apprehension confronting any object that could be described as 'headachy'.

It is arguable that the transition to perception as we know it comes through the discovery that different, implicit, proprietary appearances are comings to light of *one and the same* object. Since objects appear under conditions of appearing which vary for the different proprietary sense-fields, the classes of appearances of objects corresponding to the different modalities exhibit certain fundamental properties. Some of these properties, though characteristically different, are complementary and therefore correlatable. This is particularly true of the spatial properties of the visual and tactual groups. There is reason to think that it is through the correlation of these properties, in the first instance, that, from being merely susceptible of sensations of various sorts, the organism becomes apprised of its

physical surroundings in various ways. For the physical object is a tridimensional spatial structure, with the evidence or tokens of its solidity distributed between the spatial properties of its visual and tactual appearances. These tokens of tridimensionality must exist pre-perceptually, but in a form of which it is extremely difficult to give an accurate phenomenology without reading acquired meanings into the primitive experience. I doubt very much whether, prior to the discovery that it is the same objects that we are both seeing and touching, proprietary visual appearances have the explicit spatial character describable as 'looking flat'. For it is difficult to see how otherwise they could ever come to look solid. And it is an admirable rule of Professor Blanshard's that we should "... so construe the world we first live in as to make escape from it conceivable". Borrowing an expressive metaphor of Shadworth Hodgson's, what I think we should be saying is that pre-perceptually there is an 'expectant' solidity in the explicit look of the visual appearances and in the explicit feel of the tactual ones. Their uncoalesced tokens of tridimensionality are, as he puts it, "fragmentary, incomplete and enigmatical" but "expectant of reunion". For "there is in each case a want which the other can supply". I do not propose to ask how their reunion is effected. The essential point is that, however effected, it brings into existence a coalesced visuo-tactual field of which the sublated proprietary components 'complicate' and modify each other's deliverances in such a way that what is henceforth seen is *seen* as having, not only the sensible qualities that are proprietary to sight, but also the qualities that define tactile solidity. It is important to grasp the full significance of this point. Supporters of the Sense Datum Theory would admit that the objects which we ordinarily perceive are accessible to more senses than one and that their neutrality in this respect is part of what is meant by calling them physical. But the 'complication' of sense-fields has a more radical consequence than this. It so enriches the perceptual picture that, for a sense like sight, the object appears *to that sense alone* clothed in an assorted complement of sensible characters. The following analogy may make my meaning clearer. Gestalt psychologists and others have shown that allowances for certain distortions of the retinal image are incorporated in the seen object and not, as was previously supposed, discursively read into this. Allowances have to be made, for example, for perspectival distortions of visual shape and for the rapid diminution of the retinal image of a receding object. But a penny which turns over from the vertical

to the horizontal position as it recedes, despite the increasing ellipticity of the appearance presented by it and the rapid diminution of the retinal image, continues to be actually seen, at any rate within the range of stereoscopic vision, both to be round and to retain its standard size. And the point I wish to make is that somewhat as objects seen by the mature perceiver incorporate these corrections of the retinal image, so they incorporate for vision alone, that is, at times when we are seeing but not touching them, the tokens of their tangible solidity. A gatepost from which, at the moment, we are only receiving visual stimuli does not appear clothed with the evidences of tridimensionality that are proprietary to sight alone. A correct phenomenology demands that we describe it as literally *looking* tangibly solid. Mr. Firth quotes an observation of Dewey's that this overlap of sense-fields need not clash with any known physiological facts. This is surely an understatement. There is cumulative neuro-physiological evidence concerning the possibilities of sensory integration at the thalamic and thalamo-cortical levels which strongly suggests that the 'complication' of proprietary sense-fields, going beyond their mere coexistence in the perceptual field, is achieved centrally at these levels.

In what form then do the proprietary appearances occur at the perceptual level? Physical objects no longer appear merely implicitly with their complement of sensible qualities parcelled out between a number of uncoalesced sense-fields. Appearings of physical objects are now perceptual and therefore explicit. But proprietary appearances of them must still be occurring as sublated components within these perceptual appearances. It is always possible to pick out the sensible qualities that belong to a physical object as presented, say, through visual stimulation alone. However true it may be that I am somehow seeing the smoothness or tangibility of an object, I do recognise on reflection that it is only its colour, its visual shape and its visual solidity that I am literally seeing. Nevertheless, I am unable actually to see the object except as appearing in such wise that what thus literally appears to sight alone is only isolable by abstraction and is never normally a component of which I am separately conscious in the experience.

At no point, therefore, have we found it necessary to suppose within perception a level of awareness with special status objects. Indeed no *objects* other than physical objects have appeared in our analysis at all. Pre-perceptually, there is simply a set of uncoordinated sense-fields internally occupied by *implicit* (though 'expectant') appearances of physical objects. Then, in

mature perception, these proprietary appearings are taken up, in the way I mean when I describe them as sublated components, into the explicit, coalesced, appearings of physical objects. They continue internally to occupy our proprietary sense-fields. Perception would otherwise have no internal accusatives. But it is impossible, on my view, that in normal circumstances they should also occupy the perceptual field presentationally—which is the same as to deny that an implicit awareness of proprietary objects is uncoverable in ordinary perception.

Yet this is the claim that the Sense Datum Theorists are making. They tend significantly to elucidate the difference between a sense datum and a physical object by associating with the presentation of the former a certain quality of *immediacy* or *directness*. The fact that the sense datum is an odd kind of object they express either by saying that our awareness of it is a '*direct* awareness' or by transferring the peculiarity to the field in which it appears and saying, for example, that visual sense data occupy, not the visual, but a certain *direct* visual, field. Yet they seem unable to explain what this property of directness might really be. Thus in *A Reply to My Critics*,<sup>1</sup> Moore is at pains to show that when in the ordinary sense of 'see' one is seeing a physical object, one is necessarily also seeing something in a special sense of 'see' which he calls '*directly* see'. But he admits that he cannot explain what other sense of 'see' he can be thinking of except by giving examples of cases where 'see' is used in this sense. One such example is Macbeth's use of 'see' in "Is this a dagger that I see before me?" Now Mr. Firth makes the important suggestion that those who use the term 'direct awareness' and such cognate expressions seem to be connecting the directness of the awareness with a certain limitation upon the *sorts* of sensible characters that a directly apprehended object can manifest. Thus they would claim, he writes, that "when we gaze from a warm room at a distant snow-capped mountain . . . the manner in which we are conscious of whiteness in such a case is very different from the manner in which we are conscious of coldness". And they express this difference by saying that our awareness of the whiteness is a case of what they mean by direct awareness whereas our awareness of the coldness is not. I agree with Mr. Firth in this but would rather say that what I think the Sense Datum Theorists are really claiming is that when, for example, I see a teacup, my awareness of its colour and visual solidity

<sup>1</sup> In *The Philosophy of G. E. Moore*, Schilpp; pp. 533 ff. I shall refer to it in what follows as the *Reply*.

may properly be described as direct whereas my awareness of its tangible solidity (unless I am actually touching it) may not. In other words, the full analysis of 'directly apprehended' is 'apprehended as having only the sensible qualities that are proprietary to some one sense'. It is partly, though not entirely, in this sense that Moore is using the expression 'directly seen object'. He means by it an object seen as having only the sensible qualities that are proprietary to sight—an object *not* seen, that is to say, as having, in addition, the complementary tactual tokens of tridimensionality. This is evident from the example cited. It is clearly the failure of the dagger-like object which he was 'seeing' to be such as could be *seen* to be tangibly as well as visibly solid that accounts for Macbeth's puzzled query. 'Directly seen object', in other words, means the same as 'not a visuo-tactual object'. When Moore says that, if he is seeing his hand, it is impossible that his *hand* should be contained in his *direct* visual field, he means by 'direct visual field' precisely what I mean in this paper by the *proprietary* visual field.

I say that this is 'partly, but not entirely' the sense in which Moore is using the expression 'directly see' for the following reason. It has always been Moore's intention, he tells us, so to use the term 'sense datum' ". . . that it should not be, at all events, an *obvious* contradiction to say that some physical realities are *also* sense-data". And in the *Reply*, he not only repeats that it still is not obvious to him that there is a contradiction in saying that such physical realities as surfaces are directly apprehended, but confesses to a strong inclination to take the view that physical surfaces *are* directly apprehended. But he cannot now be using 'directly apprehended' in the sense I am imputing to the users of this phrase. Mr. Firth mentions a further proposition that these writers are agreed about, namely, that "we are never directly aware of physical objects". But the truth is that this proposition necessarily follows if we take 'directly apprehended' to mean 'apprehended as having only the proprietary qualities of some one sense'. For, obviously, if the minimum requirement of a physical object is that it should be a visuo-tactual solid, there is a contradiction in supposing that a *physical* surface could be presented, say, to sight, as having only the sensible qualities that are proprietary to that sense. There appears then to be a concealed complexity in Moore's use of the expressions 'direct' and 'directly'. It seems to me that when he is saying that he now thinks a *physical surface* may be directly apprehended, he has in mind the more

ordinary definition of 'directness' according to which 'direct awareness of an object' means 'awareness of an object such that nothing is interposed between this object and the mind that is aware of it, or such that the mind in question does not become aware of the object through, or on account of, its awareness of something else'. There is evidently no contradiction in supposing that a physical surface may be directly apprehended in this sense. Indeed I am arguing in the present paper that this is precisely how I think physical surfaces are apprehended. Since, however, Moore does not suspect that he is sometimes using 'direct apprehension' for 'apprehension of a proprietary object', he completely fails to distinguish the two senses of the phrase and thus writes, for example, as if it were immaterial whether we say that certain physical realities may be directly apprehended or say that they may also be sense data. But, on my view, it is only in the literal sense of being apprehended without an interposed medium that a physical object may be directly apprehended. It is impossible that it should be directly apprehended in the special sense in which 'directly apprehended object' means the same as what I am suggesting Moore means by a sense datum, namely, an object which is presented as having only the proprietary qualities of some one sense. But I shall return to this ambiguity in my concluding paragraph and meanwhile make my own meaning clear by adopting the following convention. When I am imputing to Moore's use of 'directly apprehend' the implication of 'apprehend as having only the proprietary qualities of some one sense', I shall write 'directly apprehend' and, similarly, 'directly perceive' or 'directly see'. Otherwise where this construction clearly cannot be put on his use of the phrase, as when he grants the possibility that we may be directly apprehending the physical surfaces of objects, I shall simply write directly apprehend or directly see (without quotation marks).

But if the definition of 'sense datum' is "any 'directly apprehended' object" and 'directly apprehended' means the same as 'apprehended as having only the sensible qualities proprietary to some one sense', it follows that in ordinary perceiving there is no such awareness as 'direct apprehension' and no such objects as sense data. For the only elements in a perception which can be said to reveal nothing but the sensible qualities that are proprietary to a given sense are such of its internal accusatives as are proprietary to the sense in question. And since these are not objects but appearances of physical objects, nothing is being 'directly apprehended' when a physical

object is being perceived and 'direct apprehension' is merely the name of the kind of awareness we should be having of the proprietary appearances if *per impossibile* these had been objects and not appearances of objects. And if nothing is being 'directly apprehended', there are *ex hypothesi* no sense data. The 'sense datum' then is a fiction. What is the origin of the fiction? The view I would submit is that sense data are the products of a fallacious alienation of the internal accusatives of perception. To suppose, for example, that my teacup percept conceals a 'visual sense datum' is to treat as a thing *seen*, and hence as another object, something which is only involved in my seeing the teacup in the way my jumping a jump would be involved in my jumping a ditch.

I set out not only to locate the origin of this fictitious object but also to inquire into the cause of its being so plausibly mistaken for a genuine object. What is my answer to this second question? There is another type of case in which Moore thinks 'see' is being used in the sense of 'directly see'. He writes: "It sometimes happens that if, after looking at a bright object, you close your eyes, you have, while your eyes are shut, an after-image of the object. And it is a quite correct use of 'see' to say that you then *see* . . . an after-image. . . . The sense in which 'see' is used here is the sense with which I am concerned."<sup>1</sup> It seems to me significant that in his latest pronouncements on the problem of perception, Moore is noticeably preoccupied with the phenomenon of after-images.<sup>2</sup> There has been little comment on this, but I believe it has a direct bearing on the answer to this second question of mine. I have hitherto only mentioned two forms in which the proprietary appearances occur in experience, *viz.* (1) pre-perceptually, as only 'expectantly' the appearing of anything physical, and (2) ordinarily, as sublated within the perceptual appearances of physical objects. Now I believe a clue to my second question lies in the fact that, alongside a percipient's normal visual perceptions, there are fringe-phenomena like double-vision and after-sensation in which certain of his proprietary visual appearances occur in an *uncoalesced form at the perceptual level*. The view I would submit is that the unconscious analogy of these rarer perceptual forms has both facilitated and at the same time camouflaged the fallacious alienation of the internal accusatives of ordinary perception. If this view is true, when in the *Reply* Moore replaces his famous directions for picking out the sense datum

<sup>1</sup> *Reply*, p. 629.

<sup>2</sup> I refer to the *Reply* and *Proof of an External World*.

in a perception of one's hand by directions for obtaining an after-image 'with the eyes closed', he is really doing more than simplifying the problem of telling his reader how to find a specimen of the sort of thing he means by a 'sense datum'. He is uncovering an unsuspected source of the persuasiveness of the sense datum theory.

The after-images Moore mentions in his examples are mainly of the positive or homochromatic variety. Positive after-images, we are told, "are due to the persistence of a state of excitation in the retina with the result that impulses continue to be discharged in the optic nerve after the removal of the stimulus".<sup>1</sup> They differ, that is, from negative after-images, which are the effects of the exhaustion or the repair that immediately follows the peripheral excitation, in being qualitatively identical with the original sensation. Thus if we stare at a luminous object such as the setting sun for a little while, the positive after-image which we then obtain when we close our eyes is literally a brief continuation of the appearance which the sun's disc was presenting to us when we were looking at it. What we are 'seeing', in the sense of 'directly seeing', is a brief prolongation in time of what we were seeing, in the ordinary sense of 'seeing', a moment before. Phenomenologically then the positive after-image will be a proprietary visual appearing of the sun's disc which has, so to speak, overshot its moment of actuality. On my interpretation of the phrase 'directly see', it is of course analytic that the after-image will be a *proprietary* visual object if, as Moore thinks, the sense of 'see' in which we see it is 'directly see'. But the same conclusion may be inferred from certain observable peculiarities of after-images. One noticeable feature is their curious lack of relation with the coalesced visuo-tactual field. This is best seen in the case of the negative after-image which is 'seen' with the eyes open and can be projected on to the surfaces of physical objects. Although 'seen' in some sense, therefore, among the physical objects of our surrounding space, the apparition never gains a foothold if that space considered as a causally interacting system. Indeed it both can and cannot be said to be seen to occupy the same space with physical objects. I suggest that this is because it is really 'seen' in an uncoalesced visual space which only coincides with the visuo-tactual space of the perceptual field in so far as the latter contains the proprietary visual field as a sublated component within it. In short, the after-image floats about in a space of its own that is only *visually*

<sup>1</sup> E. G. Boring *et al.*, *Introduction to Psychology*, p. 546.

coincident with physical space. Another analytic consequence of my view is that a proprietary visual object should fail to look solid. And inspection confirms that after-images do have a peculiar look. It is in this, among other respects, that they differ phenomenologically from images proper. Thus, the so-called 'primary memory image' reproduces the entire remembered percept in a coalesced visuo-tactual field—reproduces it, I mean, as an object which is *seen* (in memory) to be tangibly solid. But the after-image, as Ward observes, is "... more elementary than either the preceding percept or its (memory) image. In both these, in the case of sight, objects appear in space of three dimensions, *i.e.*, as geometrical solids in perspective; but the after-sensation lacks all this detail."<sup>1</sup> When, therefore, it is laid down that the positive after-image is qualitatively continuous with the original sensation, it should be noted that this continuity extends only to the colour and superficial configuration of the image. The two phenomena are radically discontinuous in one respect; as it passes into a positive after-image the object ceases to be explicitly solid-looking.

I have described the positive after-image as, phenomenologically, the survival of a proprietary visual appearing. Now this, on my view, should involve the radical consequence that after-images are not themselves objects at all, but a certain class of appearances of physical objects. But surely an after-image is something which one *sees*, however true it may be that one is seeing it in the special sense of 'see' called 'directly see'? Before I try to meet this difficulty, I might show how the supposition that, intrinsically, after-images are not objects of a peculiar sort but appearances of objects of the ordinary sort, may point to the solution of one of Moore's puzzles. He is inclined, he tells us, to think "that no after-image . . . can possibly exist except while it is being directly apprehended . . .", but is puzzled to understand "why there should be a contradiction in supposing the opposite". "I cannot see", he writes, "*where* the contradiction lies."<sup>2</sup> But if an after-image is intrinsically an appearing of some physical object in the proprietary visual field, then it is plainly a contradiction to suppose that it could exist other than as an appearing, that is, other than in so far as someone is perceiving something. Admittedly, what Moore says he is inclined to think is not that no after-image can possibly exist except while *something* (a physical object) is being directly apprehended, but that none can possibly exist except while *it itself* is being 'directly apprehended'. But the

<sup>1</sup> *Psychological Principles*, p. 174.

<sup>2</sup> *Reply*, p. 660.

fact is, as will appear in the next paragraph, that uncoalesced proprietary appearings occurring in the form of after-images do have the *prima facie* standing of objects which we 'directly see'. Moore, therefore, naturally read the inability of such a proprietary appearing to exist except in so far as something appears, as inability to exist except in so far as it itself is 'directly apprehended'. It is because of the former inability that there actually is a contradiction in supposing that an after-image could exist extra-mentally. But it is because the former inability is, as it were, concealed in the latter, that Moore is puzzled by the whole matter and unable to see where the contradiction lies.

Now it is, of course, obvious that an after-image is something *seen*, whether with the eyes closed or open. And anything seen occupies the visual field in the sense of 'occupy' which I express by 'presentationally occupy'. It is nevertheless my view that a positive after-image of, say, the sun's disc is, somehow, a brief persistence of the sun's internal occupation of some visual field beyond the point of cessation of its presentational occupation of that field. The question then becomes, How does this internal occupation of the visual field itself come to occupy that field presentationally? What is it that effects this natural alienation of a certain class of internal accusatives of perception? The answer I would suggest is in two parts: (i) By being either spatially or temporally displaced from their present perceptions, these proprietary appearings are no longer entangled in the function of cognate accusatives to those perceptions. (ii) At the perceptual level, an appearing thus cut adrift is automatically objectified.

I have suggested that the phenomenological coalescing and overlapping of sense fields is probably a function of some central integrative activity at the thalamic or thalamo-cortical levels. On the other hand, proprietary appearings are functions of the uncoordinated activity of the separate receptor mechanisms. If, therefore, any proprietary visual appearing is to outlast its parent perception and thus become temporally displaced from its object, this must take place through some property of the peripheral receptors acting independently of the higher integrative centres. The visual mechanism seems to me to involve two such properties. The one I shall deal with first effects a spatial rather than a temporal segregation of the proprietary appearing. Since we have two retinæ placed at some distance apart, the proprietary visual field is not strictly speaking simple but composite; it is compounded of two perfectly coincident or

coterminous ocular fields. In reality, therefore, there are two visual appearances of every seen object. But, so long as the ocular fields remain coincident, these appearances will be indiscernible, that is, phenomenologically they will constitute a single appearing of the physical object. It sometimes happens, however, that the two fields temporally fail to coincide, with the result that the two appearances of the object are drawn apart and the phenomenon of double vision occurs. Now I find on careful scrutiny that one of the two images in double vision may be picked out as noticeably more spectral or wraith-like than the other. It is as if this image were a duplicate of the *proprietary* visual appearing which is contained as a sublated component in the perceptual appearing of the object. If this is indeed the case, we have here a situation in which the proprietary visual appearing of an object is spatially displaced from its parent perception and freed of its internal involvement in this as cognate accusative to the seeing. The second property of the peripheral visual mechanism I have already mentioned. It is that the state of excitation in the retina set up by external stimulation may persist after the withdrawal of the stimulus, with the result that impulses continue to be discharged in the optic nerve. When this happens we obtain a positive after-image. And this, I have submitted, is, phenomenologically, a proprietary visual appearing of some luminous object made to outlast its parent perception and thus temporally displaced from the object which is, or rather was, appearing. As such it is cut adrift from its involvement in the parent perception as cognate accusative to the seeing.

Here then are two cases where an actual displacement of the proprietary visual appearing of an object takes place—one a spatial and the other a temporal displacement. But in adult perception the matter cannot end there. A proprietary appearing so displaced cannot retain its intrinsic character of an internal occupation of the proprietary visual field. It is automatically objectified, that is, made to occupy the proprietary visual field presentationally. From being a certain odd sort of 'seeing', it becomes itself an object seen. And inasmuch as the field it presentationally occupies is the *proprietary* visual field, the sense in which it is seen will be, by definition, the special sense of 'see' expressed by 'directly see'.

Now in view of these admissions, the sense in which I deny that 'direct apprehension' occurs or that sense data exist will have to be more carefully defined. Moore so uses the expression 'sense datum' that *anything whatever* that is 'directly

perceived' must be a sense datum. It follows from the foregoing analysis that after-images in that case are sense data. And when, therefore, I deny that sense data exist, I cannot be meaning to deny that *certain* sense data, in Moore's sense of the term, exist. But more commonly the term 'sense datum' is used in another sense, to mean an object, complying with Moore's definition, that is believed to be uncoverable in the perception of perfectly ordinary things like tables and teacups. It is the existence of this class of sense data that I consider dubious. I doubt, that is to say, whether ordinary percepts are resolvable into constituents of which the peculiarity is that they are proprietary or 'directly perceived' objects. I do not deny that at the fringes of perception certain such 'objects' are discoverable. Actually these are not true objects but objectifications of certain proprietary appearances made available for objectification by displacement from their parent perceptions. But the fact that they do constitute *prima facie* objects has had a pernicious effect on the analysis of perception. I set out to examine the claim that an operation of 'perceptual reduction' is possible. My submission is that 'reducing' an ordinary percept simply consists in imagining what it would be like for its proprietary appearances to become uncoalesced and turned into an order of special status objects of which, being proprietary, the peculiarity will be that they are perceived in the special sense of 'perceive' expressed by 'directly perceive'. And it is because epistemologists possess the analogy of proprietary appearances which have undergone a natural objectification and actually occur in their experience as *prima facie* objects, that they *can* imagine what it would be like for the proprietary visual appearances of tables and teacups to be similarly objectified.

I admitted earlier that the proprietary appearances of physical objects did once coexist in an uncoalesced form, *viz.*, at the pre-perceptual stage in the maturation of consciousness. Why then should it not be possible, by a properly directed effort of attention, to uncoalesce them again? Why, in other words, do I hold that perceptual reduction must be an imaginary operation? My answer is that the Sense Datum Theorists are unable actually to resolve their ordinary percepts into uncoalesced proprietary appearances because of a fundamental natural resistance to the *uncoalescing of the visual and tactual sense-fields* which such a general resolution of the perceptual picture would involve. And I suggest that there is bound to be a powerful unconscious resistance to any attempted uncoalescing of the visual and tactual sense-fields because the consequent radical

decomposition of the perceptual field would undermine the whole structure of perceptual consciousness considered as a cognitive process, involving the presentation of the physical object to a percipient Subject or Self. In making this suggestion, I am presupposing a certain view of the significance of the personal pronoun in propositions of the form 'I am perceiving such and such a physical object'. As I have fully discussed this view in a previous paper, a summary statement of it will here suffice. The view directs attention to a *third* sense-field of equal importance with the visual and the tactual, viz., the *somatic* sense-field. The sole occupant of this field is one's own body as interoceptively experienced. Now there exists one such somatic sense-field and one visual sense-field which have, for me, the unique property that I am able to refer to them, respectively, as '*this* somatic sense-field' and '*this* visual sense-field' using '*this*' as a logically proper name. These two sense-fields I shall designate *S* and *V* respectively. Now *V* is related to *S* by a relation of spatial continuity such that, if I happen to be seeing parts of the surface of my own body, certain appearances of minimal depth in *V* will be sensibly continuous with *S*, whereas, if I am not seeing any part of my own body, *V* will be spatially continuous with an obtainable visual field which, if actualized, would be sensibly coincident at its extreme front in the same manner with *S*. And I hold that it is the sensible presence of *V* to *S* in this literal, spatial, sense that I am in fact expressing when I say of any object that appears in *V* not that it is seen now but that *I* am seeing it now.

If *S*, however, is to be spatially continuous with *V*, it must be completely contained or immersed in it. For the 'I' which perceives does not contemplate the physical world from without but occupies a point of view within it, in that important second sense of 'point of view' which Professor Price defines as 'position in the system of standard solids'.<sup>1</sup> The occupation of this point of view is only partly a visual experience. I *feel* as well as see myself occupying a region of the space in which I am also perceiving other bodies. In so far, at least, as the perceiving Self is concerned, the sense of selfhood involves a fundamental sense of location, which is a composite somato-visuo-tactual experience. If, therefore, as I claim, the visual presence of an object to me is analysable in terms of an elemental relation of spatial continuity between *V* and *S*, and the spatial relation is to be of the right sort, then *S* must be brought into the visual field as an occupant of it. Would any conditions then be attached

<sup>1</sup> *Perception*, p. 253.

to such containment? Clearly this will depend on the proprietary characteristics of the two sense-fields concerned. *S* is a voluminous spatial whole extended in three dimensions with a continuous outer surface coinciding with the surface of my skin. Its tridimensionality is a *proprietary* quality of it. I mean, it is not a function of different complementary modalities, like the volume of an external body, but is manifested entirely through the somatic or interoceptive sensibility itself. On the other hand, I have argued earlier in this paper that *V*, if by *V* we mean the *proprietary* visual field, is not explicitly tridimensional but has the quality which I have called the 'expectation of tridimensionality'. But if *S* is intrinsically a volume, its *complete* containment in *V* would have to amount to containment in three dimensions, which simply means that *S* cannot be contained in the manner required in the uncoalesced proprietary visual field. This field would not have the explicit third dimension in which to accommodate the sensible voluminousness of *S*. On our analysis, therefore, there is only one sort of visual field in which *S* can be tridimensionally accommodated—accommodated, that is, in the way required to constitute it the Subject of visual experiences, namely, a visual field which has realized its expectation of tridimensionality. In other words, *S* can be suitably contained in *V* only if by '*V*' we mean, as we do ordinarily mean, the *coalesced visuo-tactual sense-field*.

It seems to me that, phenomenologically, the whole structure of mature perceiving rests on two correlated processes—the process of constituting the somatic sense-field the Subject of perceptual consciousness, and the process of constructing a perceptual or coalesced field on the basis of the fusion of the complementary deliverances of vision and touch. And the resistance to the attempt to *uncoalesce* the visual and tactual components of this field, which is involved in any effort to abstract and objectify the proprietary appearances of physical objects, comes from the *conatus* by which the percipient Subject, once constituted, *in suo esse perseverare conatur*. One of the most serious mistakes of the Sense Datum Theorists is to think that they can retain the '*I*', with its full complement of perceptual automatisms and capabilities, over against a radically decomposed perceptual field.

There is one concluding application of the thesis of this paper that I am tempted to make. It concerns another of Moore's puzzlements. In subsection 10 of the *Reply*, he confesses that he now thinks he was wrong in arguing, as he had done in *The Refutation of Idealism*, "that in *no case* is the *esse* of anything

*percipi*". Then follows this extraordinary passage: "I am now very much inclined to think that . . . it is as impossible that . . . *anything whatever which is directly apprehended*, any *sense-datum*, that is, should exist unperceived, as it is that a headache should exist unfelt. If this is so, it would follow at once that *no sense-datum can* be identical with any physical surface, which is the same thing as to say that no physical surface can be directly apprehended; that it is a contradiction to suppose that any is. Now at the end of the last section I said that I was inclined to take the view . . . that physical surfaces *are* directly apprehended. I am, therefore, now saying that I am strongly inclined to take a view incompatible with that which I then said I was strongly inclined to take. And this is the truth. I am strongly inclined to take both of these incompatible views. I am completely puzzled about the matter."<sup>1</sup>

Now plainly, either there is an unresolvable contradiction here, or the two views in question are only seemingly incompatible. But the incompatibility is only apparent if, as I suggest, Moore is really using the expression 'directly apprehended' in two different senses. My submission, then, would be that the reason why he favours the view that physical surfaces *are* directly apprehended is that he is really thinking here of the view that nothing is interposed between the mind and the actual surface of the perceived body. And his strong inclination to take *this* view simply reflects his greater reverence, as compared with other Sense Datum Theorists, for the phenomenological facts. For if, as I claim, Moore does not exist as a percipient except correlatively to an unresolved perceptual field, it is a condition of the very possibility of his scrutinizing a physical object, that he cannot do this with an open mind and fail to be overwhelmingly convinced that it is the actual, visuo-tactual, surface of the object that he is perceiving. On the other hand, the reason why he is *also* inclined to take the seemingly incompatible view that there would be a contradiction in supposing that physical surfaces are 'directly apprehended' is that he is *now* using 'directly apprehended' in the *other* sense, that is, with the implied meaning of 'apprehended as having only the qualities proprietary to some one sense'. The reason he gives why there should appear to him to be this contradiction is that he now thinks it *impossible* that anything whatever which is 'directly apprehended' should exist except while it is being apprehended. And he is puzzled to understand

just why he should think this. But, if my analysis has been correct, there is a very good reason why he should think it. Suppose I am right and Moore is now using 'directly apprehended' with the implied meaning of 'apprehended as having only the qualities proprietary to some one sense'. Then I would argue thus ; an object, supposed existing, which is limited as to its qualities in this way, is not intrinsically an object at all, but some physical object's proprietary appearing projected as an object. And there is an obvious contradiction in supposing that there could occur, say, a visual appearing from the occurrence of which it did not follow that someone was then seeing something. There would be the same contradiction in supposing the opposite as there would be in supposing that there could occur a stroke such that it did not follow from its occurrence that someone was then striking something. There is indeed the difficulty that what Moore *says* he is now inclined to think is that it is impossible that a 'directly apprehended' object should exist except while *it itself* is being apprehended. To this I give the answer I gave earlier to the objection that what Moore actually says when he makes the same point about the after-image is that he feels sure that an after-image could not possibly exist except while it itself was being 'directly seen'.<sup>1</sup> My answer, that is, would be that, despite his phenomenological rectitude, in granting even the possibility of a sense datum analysis of ordinary perceptions, Moore has involved himself in the imaginary objectification of the proprietary appearances of physical objects—the only elements in ordinary perception to which, once the objectification is made, the description 'directly apprehended objects' can apply. For, once these internal accusatives of perception are objectified, the inability of a proprietary appearing to exist except in so far as *something* appears, is naturally read as inability to exist except in so far as it itself is 'directly apprehended'.

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<sup>1</sup> The two issues, on my analysis, are, of course, the same. And in fact what Moore actually says in the passage on page 660 is that he is "inclined to think that no after-image (*and, therefore, also no 'sense-datum'*) can possibly exist except while it is being directly apprehended". (My italics).

## V.—ON SOME SEMANTIC ILLUSIONS

BY ELLIS EVANS

RUSSELL says on page 39 of *Principia Mathematica* 'When we say that " $\phi x$ " ambiguously denotes  $\phi a, \phi b, \phi c$ , etc., we mean that " $\phi x$ " means one of the objects  $\phi a, \phi b, \phi c$ , etc., though not a definite one, but an undetermined one. It follows that " $\phi x$ " only has a well-defined meaning (well-defined, that is to say, except in so far as it is of its essence to be ambiguous) if the objects  $\phi a, \phi b, \phi c$ , etc., are well-defined. That is to say, a function is not a well-defined function unless all its values are already well-defined. It follows from this that no function can have among its values anything which presupposes the function, for if it had, we could not regard the objects ambiguously denoted by the function as definite until the function was definite, while conversely, as we have just seen, the function cannot be definite until its values are definite. This is a particular case, but perhaps the most fundamental case, of the vicious-circle principle.'

Now when Russell says that no function can have among its values anything that presupposes the function, he must primarily mean, that no *variable* can have among its values anything that presupposes the function that contains it, for in any function it is the variable which actually "ambiguously denotes". His reason for this is, that if it did, the objects  $\phi a, \phi b, \phi c$ , etc., would not be well-defined until  $\phi x$  were well-defined, whereas in fact the converse must be the case. Now  $\phi a, \phi b, \phi c$ , etc., are themselves not well-defined unless  $a, b, c$ , etc. (the values of the variable) are well-defined. So his argument comes to this: that whereas  $a, b, c$ , etc., must be well-defined before  $\phi x$  is well-defined, the consequence of one of those values  $a, b, c$ , etc., being  $\phi x$  (or anything that contains or presupposes it) would be that  $\phi x$  would have to be well-defined before  $a, b, c$ , etc., were well-defined. Thus a variable cannot presuppose the function which contains it, because that would imply the prior definition of the function.

The antithesis on which this argument depends is between the dependence of the definition of  $\phi x$  on the definition of  $a, b, c$ , etc., on the one hand, and the converse dependence on the other. Now he says that  $a, b, c$ , etc., must be defined before  $\phi x$  can be defined.

But does this mean that the values of  $x$  must be defined

before  $\phi$  is defined? Clearly not: in the function ' $x$  is hurt', ' $\text{is hurt}$ ' is immediately comprehensible: but in any case the order of comprehension as between  $x$  and  $\phi$  is immaterial. Consequently, in the converse situation, which he says the presupposition of the function by its variable would imply, he does not mean that it would be implied that  $\phi$  would have to be defined before  $x$  were defined.

Nevertheless, he says that the values of  $x$  must be defined before  $\phi x$  can be: but he does not mean that  $x$  must be defined before  $\phi$ : now  $x$  is a part of  $\phi x$ . Therefore he must mean that the values of  $x$  must be defined before  $x$  can be. But the definition of  $x$  consists in the definition of its values. Thus he is saying that the definition of  $x$  must precede itself. And conversely, to say that the consequence of the presupposition of the function by its variable is the definition of  $\phi x$  before that of  $x$ , is to say that that consequence is the definition of  $x$  before itself.

The fact that  $x$  is a variable is quite irrelevant. The fact that in ' $x$  is hurt' we do know what ' $\text{is hurt}$ ' means and are not sure of everything that  $x$  means, is irrelevant to the fact that in order to know what ' $x$  is hurt' means we have to know what  $x$  means, just as it is irrelevant to the fact that we have to know what ' $\text{is hurt}$ ' means before we can know what ' $x$  is hurt' means. The fact that  $x$  is a variable and  $\phi$  is not is irrelevant to the fact that if you have mentioned the definition of  $\phi x$  (which is both) you have mentioned the definition of  $x$ .

Thus to say that a variable cannot presuppose the function which contains it, because that would imply the prior definition of the function, whereas the variable must be defined before the function can be, is to give no significant reason: for at the moments at which the definition of the function is referred to, the definition of the variable is also referred to, and one is saying that what would be implied is that the variable must be defined before the variable can be, whereas the variable must be defined before the variable can be.

What seems to have occurred is an unconscious hypostatization of the  $\phi x$  which is one of the objects  $a, b, c$ , etc., as a mere mark. If that were to happen, namely that the  $x$  in  $\phi x$  had as a value the mark  $\phi x$ , that would certainly mean that that value of  $x$  was well-defined: if then one mistakenly identified that mark  $\phi x$  with the original function, one would judge that  $\phi x$  had been defined before the variable had been. But they cannot, of course, be so identified, for the original function has meaning, whereas a mere mark has none.

*Self-reference and the 'illegitimate totality'*

He continues: 'A function is what ambiguously denotes some one of a certain totality, namely the values of the function; hence this totality cannot contain any members which involve the function, since, if it did, it would contain members involving the totality, which, by the vicious-circle principle, no totality can do.' It is obviously the variable which gives a function its so-called ambiguity: so the argument may be re-phrased; a variable is what ambiguously denotes some one of a certain totality, namely the values of the variable; hence this totality cannot contain any members which involve the variable, since if it did, it would contain members involving the totality, which no totality can do.

Now the notion of a member 'involving' the variable is not a very well-defined one, since there are many ways in which one thing may involve another. It will be better, then, to consider the essential situation, whereby the totality includes the variable, rather than the generalised and ambiguous situation of the totality including some member which in some way involves the variable. We shall therefore take the argument as primarily attacking the notion that the totality may include the variable itself, on the grounds that if it did, it would include something which involved the totality.

Now as long as the variable is ambiguous, *i.e.* is in fact a variable, the number of members constituting its range of values is clearly not relevant to Russell's argument: the size of the totality does not matter. Let us then take the case of a variable which is ambiguous as to which of *two* values it denotes, and let one of these be named 'a', and let the other one be the variable itself. Then the argument will run that the totality of what x denotes (which consists of a and the variable) includes something, namely the variable, which involves the totality.

Now what is it for a symbol to denote itself? Any particular symbol has three aspects: first, its graphic properties as a mark (supposing it to be visual)—*e.g.* its shape and colour: secondly, its property of significance: thirdly, the significance it has (*i.e.* the concepts it arouses, or the objects it denotes, etc.). First then, a symbol may denote itself as a mere mark as the following symbols do 'the marks of which this phrase is composed'. But if the variable denotes itself in this way, what it denotes does not involve any further meaning at all, let alone a totality of them. Thus if x denotes a, and in this sense, itself, it denotes a and the mark 'x', and the mark 'x' does not

have any meaning, being symbolised as a collection of graphic properties.

If *x* refers to itself in the second way, it denotes its own property of significance. Is this possible?

It seems clear that a mark can have this property only as a consequence of there existing something of which it is significant, for if not, a mark could be significant without being significant of anything, which seems impossible. Thus the possession of a property of significance by *x* presupposes the existence of something of which *x* is significant: now a property cannot presuppose itself: therefore, if *x* conveys that property, it must also convey something other than that property. Now what it conveys is itself and *a*, by hypothesis: therefore if it conveys its own property of significance, it may do so as a consequence of denoting *a*, or as a consequence of denoting itself in some sense other than this second one we are considering.

But even if *x* were to convey itself in this way, i.e. its property of significance, it would not thereby refer to anything involving the totality of its denotations: for from the fact that a symbol is significant, one cannot infer that of which it is significant. If I know only *that x* means, I still do not know *what x* means, obviously.

Neither of the two ways so far considered in which a symbol may refer to itself will satisfy the argument, then, for in neither way is any totality of meanings involved.

Now a third way in which a symbol might, at least at first sight, seem able to refer to itself, would be for it to refer to its third aspect, i.e. that of which it was significant. Now were this possible, the argument would clearly be satisfied, for then the totality of what *x* denoted would include that aspect of the variable which involves the totality.

Thus what is required is a symbol that refers to what it means. Obviously every symbol does this in one sense, for it means whatever it means. But to say of a symbol that it does convey what it does convey, is not to say what it conveys. Thus to say that the variable has itself as a value, and by 'itself' to mean, that which the variable conveys, is not to say *what* the variable conveys, but merely to say, *that* the variable conveys. We are using 'itself' as equivalent to the descriptive phrase 'what *x* conveys', and have not yet shown that anything fits that description. However, the variable conveys *a*, as well as itself. And by 'itself' we mean whatever the variable conveys. But 'whatever the variable conveys' is a description which only *a* fits: for we have seen that to take 'itself' as meaning

the variable as a mark, or as that and its consequent property of significance, or as its property of significance consequent on its denotation of a, does not involve any totality, and is not what the argument requires. Thus what  $x$  conveys, is a. But we see immediately the possibility of confusion through assuming that 'itself' or 'what  $x$  conveys' is a description which must involve something *apart from* a, a confusion which is very understandable if we do not clearly distinguish between those aspects of a symbol which do not involve what it signifies, and that aspect which does. Easy to think that if the variable refers to itself, it must refer to something: for it is something: but we have dismissed those parts of it that are irrelevant to the argument, and there is nothing left for it to refer to.

Thus the totality of the variable's values is made up of a, and Russell's argument immediately collapses, for to say that  $x$  conveys a and itself, is merely to repeat that  $x$  conveys a, unless we mean by 'itself' those aspects of the variable that do not involve what  $x$  signifies: and however many other things b, c, d, etc. make up the known values of the variable, the same considerations will apply, and to say that  $x$  conveys them and itself, is to repeat that  $x$  conveys them.

Now by ourselves making that false assumption, that some entity other than the known values of the variable must fit 'what  $x$  conveys' or 'itself', we may clearly see how a vicious circle principle has to be invoked to prevent an illegitimate totality. Given that  $x$  conveys a, b, c and itself: and assuming that the 'itself', or what  $x$  conveys, is something more than a, b, c: then  $x$  conveys (1) a, b, c, (2) something more. But that something more is what  $x$  conveys. Thus 2 is 1 and 2. Therefore the totality of what  $x$  conveys, namely 1 and 2, contains an element, namely 2, which is the totality. So we seem to need a principle illegitimising the self-reference which leads to that absurdity. However, the absurdity is not the consequence of self-reference as such, but rather of a mistaken view of the nature of self-reference: the assumption, in fact, that if  $x$  conveys itself, as well as some other things, it must convey something apart from those other things, which is wrong—unless  $x$  conveys itself as a mere mark, or as a property of significance—but in those cases the illegitimate totality cannot arise anyway.

In any case the notion of a totality is not necessary to the essential confusion. Let  $x$  be a mark which is said to denote itself: now we find out that that is not intended to mean that  $x$  conveys the *mark*  $x$ , nor that  $x$  conveys its own property of

significance. But the only characteristic that  $x$  has left is whatever it denotes. Thus the statement that  $x$  denotes itself means that  $x$  denotes whatever it denotes. But assume that is not realised, and that it is thought that it has been said, what  $x$  denotes : and assume that an attempt to evaluate the meaning of  $x$  takes place. Then :  $x$  denotes itself : but 'itself' denotes what  $x$  denotes : thus  $x$  denotes itself, which denotes itself, which . . . and there is no end. Since it is thought that ' $x$  denotes itself' tells you what  $x$  denotes, it is thought (reasonably enough on that score) that 'itself' conveys what  $x$  denotes. Thus the string of 'itself's seems legitimate enough. But in fact each occurrence of 'itself' reflects the assumption that there is something which  $x$  denotes, called 'itself'. Thus each attempt to find out what  $x$  denotes, results in one further thing that it must denote. The totality of what  $x$  denotes is in fact empty, but it is thought to contain a member : now each attempt to get at that member involves a repetition of that original wrong assumption : thus the totality of what  $x$  denotes is never complete—there is always one more thing in it than can actually be shown, which is the result of the initial mistake that there is a thing in it at all. That this confusion is independent of whatever other well-definable meanings a variable may have, is obvious : for the confusion insists that  $x$  have a meaning, when it cannot be shown to have any, and thus insists that  $x$  have fifty-seven meanings when it can only be shown to have fifty-six.

There is no need for a theory to make this process illegitimate: it was that already, arising from a lack of definition of the concepts involved in making a variable have itself as a value. It is not that the result of that operation would be unfortunate for some logical purpose, but that the concepts involved in an instruction to perform the operation cannot be organised properly : every formulation of such an instruction is semantically confused. Take for example the instruction 'let  $x$  signify itself'. When one reads that, 'itself' arouses certain notions in the mind : the notion of the graphic properties (or some of them) of  $x$ , the notion that  $x$  is to have a property of significance, so that it shall signify . . . what ? If not the graphic properties and if not the consequent property of significance, there is nothing else. The thing looks like a description of a possible state of affairs, and so it would be, if  $x$  were to denote itself in those first two senses. Failing that, the instruction does not say what  $x$  is to signify. Every formulation resulting in the so-called illegitimate totality is defective in the same way.

We shall now see that the paradoxes of the liar, heterologicality, the class of classes not members of themselves, and certain others, can be derived from semantic confusions in essence identical with that treated above. In each case, some symbol (or group of them) referring to itself, is covertly assumed to have a 'self' other than any it can be shown to have, upon which the structure of the paradox depends, and without which the paradox cannot be posed. Russell's concept of a propositional function in which the variable *ambiguously* denotes the members of its range encourages the confusion by prompting the question, what if the variable denotes itself? But if the variable  $x$  is taken as merely indicative of the possibility of the designations of its so-called values replacing it in the function (which does not imply any semantic connexion between it and those values) that question could only arise in the sense, what if one of those values were the *mark*  $x$ ? or, what if one of those values were the notion of replacing  $x$  by some value?, since there is nothing else to  $x$  but those aspects. But from neither of these questions can the confusion arise.

*Analysis of interpretations of self-referring sentences*

Assume now that an ' $x$ ' which signifies itself occurs as subject to a predication. Then there may be three notions as to what the statement is about: first, that it is about the mark ' $x$ ': secondly, that it is about the property of significance that ' $x$ ' has: third, the illusory notion that it is about what ' $x$ ' denotes, it being wrongly thought that it can denote something more. This third possibility does not, as we have seen, give a statement, since there is no subject for it: but given the confusion we have been discussing, it would be thought to be a statement about something.

Now we have seen that the second possibility can exist only if ' $x$ ' refers to itself in some other way as well. That other way may be the first way: then ' $x$ ' means the property of significance that it has consequent on its meaning itself as a mark. But given the illusion of the third way, there may be the corresponding illusion of a second. Then ' $x$ ' signifies what entity is wrongly thought to fit 'what  $x$  signifies', together with the property of significance that  $x$  has in signifying that (fictitious) entity. We have altogether then four possibilities, two the result of confusion: ' $x$ ' signifies itself as a mark: as a mark and a consequent property of significance: as a fictitious entity: as that and a consequent, equally fictitious, property of significance.

We may notate this in the following way. If mere marks are meant, we place 0 after the words concerned: thus (these symbols)<sup>0</sup> shows that the bracketed words are considered as mere marks. If a property of significance is meant, we put 1 after the words concerned: thus (these symbols)<sup>1</sup> indicates the property of significance that those words have as a consequence of denoting the marks composing them. 2 placed after such words represents the illusory self that the words are thought to signify: 3, the illusory property of significance that such words may be thought to have as a result of denoting that illusory self. Words used normally will not have a number put after them.

Then for a  $\phi x$  in which  $x$  denotes itself we have the following interpretations.

1.  $(x)^0$  has property  $\phi$ , or  $\phi(x)^0$
2.  $(x)^1$  „ „  $\phi$ , or  $\phi(x)^1$
3.  $(x)^2$  „ „  $\phi$ , or  $\phi(x)^2$
4.  $(x)^3$  „ „  $\phi$ , or  $\phi(x)^3$

Making  $\phi$  the property of being interesting and replacing 'x' by 'these symbols', 1. says that 'these symbols' are interesting (i.e. as marks); 2. that the property of significance that 'these symbols' have (by virtue of denoting themselves as marks) is interesting; 3. that whatever 'these symbols' means (which is not, however, themselves as marks nor as a consequent property) is interesting; and 4. that the property of significance that 'these symbols' have (by virtue of denoting whatever they do denote under 3—which is in fact nothing)—is interesting.

Assume now that 'x' denotes, not itself, but the  $\phi$  in conjunction with which it occurs. Then it may denote it as a mark, as a property of significance, or denote what it denotes. These possibilities give the following interpretations of the  $\phi x$  in which such an 'x' occurs.

- (a)  $(\phi)^0$  has property  $\phi$ , or  $\phi(\phi)^0$
- (b)  $(\phi)^1$  „ „  $\phi$ , or  $\phi(\phi)^1$
- (c)  $(\phi)$  „ „  $\phi$ , or  $\phi(\phi)$

Making  $\phi$  the property of being interesting, (a) says that the mark 'interesting' is interesting, (b) that the property of significance possessed by the word 'interesting' is interesting, (c) that the property of being interesting is interesting. None of these statements are illusory, since there is no self-reference involved.

Let us suppose now that the 'x' in  $\phi x$  denotes  $\phi x$ , that is, the whole of the statement in which it occurs. Then each interpretation of 'x' denoting itself will combine with each of 'x' denoting  $\phi$ . Further, there will be two possible interpretations of the structure of each of the resulting predications of  $\phi x$  by  $\phi$ : for that predication can be taken either as a single predication by  $\phi$  of a whole subject  $\phi x$ , or as a compound predication constituting in fact two predications, one of  $\phi$  by  $\phi$ , and one of  $x$  by  $\phi$ .

1 above will combine with each of (a), (b), and (c) in turn and so will 2, 3, and 4. Each combination will have two interpretations. Then we have:

- 1a (i)  $\phi(\phi x)^0$  (These symbols have  $\phi$ )<sup>0</sup> has  $\phi$ .  
 (ii)  $\phi(\phi)^0, \phi(x)^0$  (These symbols)<sup>0</sup> has  $\phi$ , and (have  $\phi$ )<sup>0</sup> has  $\phi$ .  
 1b (i)  $\phi(\phi^1 x^0)$  (These symbols)<sup>0</sup> has  $\phi^1$  has  $\phi$ .  
 (ii)  $\phi(\phi)^1, \phi(x)^0$  (These symbols)<sup>0</sup> has  $\phi$ , and (have  $\phi$ )<sup>1</sup> has  $\phi$ .  
 1c (i)  $\phi(\phi x^0)$  (These symbols)<sup>0</sup> has  $\phi$  has  $\phi$ .  
 (ii)  $\phi(\phi), \phi(x)^0$  (These symbols)<sup>0</sup> has  $\phi$ , and (have  $\phi$ ) has  $\phi$ .  
 2a (i)  $\phi(\phi^0 x^1)$  (These symbols)<sup>1</sup> has  $\phi^0$  has  $\phi$ .  
 (ii)  $\phi(\phi)^0, \phi(x)^1$  (These symbols)<sup>1</sup> has  $\phi$ , and (have  $\phi$ )<sup>0</sup> has  $\phi$ .  
 2b (i)  $\phi(\phi x^1)$  (These symbols have  $\phi$ )<sup>1</sup> has  $\phi$ .  
 (ii)  $\phi(\phi)^1, \phi(x)^1$  (These symbols)<sup>1</sup> has  $\phi$ , and (have  $\phi$ )<sup>1</sup> has  $\phi$ .  
 2c (i)  $\phi(\phi x^1)$  (These symbols)<sup>1</sup> has  $\phi$  has  $\phi$ .  
 (ii)  $\phi(\phi), \phi(x)^1$  (These symbols)<sup>1</sup> has  $\phi$ , and (have  $\phi$ ) has  $\phi$ .  
 3a (i)  $\phi(\phi^0 x^2)$  (These symbols)<sup>2</sup> have  $\phi^0$  has  $\phi$ .  
 (ii)  $\phi(\phi)^0, \phi(x)^2$  (These symbols)<sup>2</sup> have  $\phi$ , and (have  $\phi$ )<sup>0</sup> has  $\phi$ .  
 3b (i)  $\phi(\phi^1 x^2)$  (These symbols)<sup>2</sup> have  $\phi^1$  has  $\phi$ .  
 (ii)  $\phi(\phi)^1, \phi(x)^2$  (These symbols)<sup>2</sup> have  $\phi$ , and (have  $\phi$ )<sup>1</sup> has  $\phi$ .  
 3c (i)  $\phi(\phi x^2)$  (These symbols)<sup>2</sup> have  $\phi$  has  $\phi$ .  
 (ii)  $\phi(\phi), \phi(x)^2$  (These symbols)<sup>2</sup> have  $\phi$ , and (have  $\phi$ ) has  $\phi$ .  
 4a (i)  $\phi(\phi^0 x^3)$  (These symbols)<sup>3</sup> have  $\phi^0$  has  $\phi$ .  
 (ii)  $\phi(\phi)^0, \phi(x)^3$  (These symbols)<sup>3</sup> has  $\phi$ , and (have  $\phi$ )<sup>0</sup> has  $\phi$ .  
 4b (i)  $\phi(\phi^1 x^3)$  (These symbols)<sup>3</sup> has  $\phi^1$  has  $\phi$ .  
 (ii)  $\phi(\phi)^1, \phi(x)^3$  (These symbols)<sup>3</sup> has  $\phi$ , and (have  $\phi$ )<sup>1</sup> has  $\phi$ .  
 4c (i)  $\phi(\phi x^3)$  (These symbols)<sup>3</sup> has  $\phi$  has  $\phi$ .  
 (ii)  $\phi(\phi), \phi(x)^3$  (These symbols)<sup>3</sup> has  $\phi$ , and (have  $\phi$ ) has  $\phi$ .

It will be seen that 3a to 4c inclusive involve the confusion as to the variable referring to itself: and that 4a, b and c depend upon 3a, b and c, since the  $x^3$  interpretation depends upon the  $x^2$  one. Thus 3a, b and c are the primary result of that confusion.

We notice that in only two of these interpretations do we have the  $x^3$  illusion together with a within-bracket  $\phi$  used normally, i.e. not as a mark on paper, and not as a property of

significance, but in order to mean whatever property it does mean. Thus 3c (i) gives us a predication of the *same* predication of an illusory subject: while 3c (ii) gives a predication of the illusory subject, and another predication of that predicate by itself. Now, were 3c (i) further evaluated according to the  $x^2$  confusion, we should obtain further predications of the same predication of the illusion: but however far the evaluation were continued, no ultimate predication of anything definite would in fact be obtained. Given the illusion that the  $x^2$  interpretation is meaningful, we would imagine that we would have obtained a predication of the same predication of . . . something definite.

Let there be a property which can be significantly predicated of propositions, and such that, if a proposition has it, the proposition which says so hasn't, and if the proposition which says that a proposition has it, hasn't it, the proposition said to have it, has. Thus, writing 'p' for any proposition, and ' $\phi$ ' for this property, if p has  $\phi$ , 'p has  $\phi$ ' has not  $\phi$ : and if 'p has  $\phi$ ' has not  $\phi$ , p has  $\phi$ .

Then, suppose we have 'this sentence is  $\phi$ ' and take the 3c (i) interpretation. Has that sentence  $\phi$  or not? Writing 'p' for the words 'this sentence', 3c (i) tells us that "'p has  $\phi$ ' has  $\phi$ " means the same as 'p has  $\phi$ '. Now, if p has  $\phi$ , 'p has  $\phi$ ' has not  $\phi$ , by the definition of  $\phi$ . Therefore, if 'p has  $\phi$ ' has  $\phi$ , it has not  $\phi$ .

But if 'p has  $\phi$ ' hasn't  $\phi$ , then, by definition of  $\phi$ , p has  $\phi$ . But by 3c (i), 'p has  $\phi$ ' means the same as "'p has  $\phi$ ' has  $\phi$ ". Therefore, if 'p has  $\phi$ ' hasn't  $\phi$ , it has.

Falsehood is such a property as defined above, and 3c (i) shows the structure of the interpretation of 'this sentence is false' which results in the liar paradox. The paradox derives directly from the  $x^2$  illusion.

Let us now consider statements in which the predicate, not the subject, denotes the whole statement in which it occurs: that is, a  $\phi x$  in which the ' $\phi$ ' denotes  $\phi x$ . To distinguish the structure of such statements in which the predicate symbols have a variable denotation, while the subject remains constant, from the converse structure, we shall call such a structure  $x\phi$ , not  $\phi x$ : what then could  $x(x\phi)$  be interpreted as meaning?

Let R be the relation holding between the subject of a statement and the property or properties being attributed to it by the statement. Then there would seem to be two kinds of interpretation of the structure of  $x(x\phi)$  corresponding to (a)  $xR(xR\phi)$  and (b)  $xRx$ ,  $xR\phi$ . In words the two interpretations

correspond to (a)  $x$  has the property such that  $x$  has the property  $\phi$ , and (b)  $x$  has the property  $x$  and  $x$  has the property  $\phi$ .

In order that it may refer to  $x\phi$ , ' $\phi$ ' must refer to itself. This may be as a mark, as its property of significance in consequence, as an illusory entity satisfying the description "what ' $\phi$ ' conveys" or for instance 'this property', and as a property of significance by virtue of this illusory self-reference.

If the first two cases are to be followed up, it must be thought possible that a mark should constitute a property. Let us assume this is thought possible.

Thus analogically with our previous treatment of the self-referring ' $x$ ', we have the following four interpretations of an  $x\phi$  in which ' $\phi$ ' denotes itself:

5.  $x$  has property  $(\phi)^0$ , or  $x(\phi)^0$
6.  $x$  " "  $(\phi)^1$ , or  $x(\phi)^1$
7.  $x$  " "  $(\phi)^2$ , or  $x(\phi)^2$
8.  $x$  " "  $(\phi)^3$ , or  $x(\phi)^3$ .

Let now the ' $\phi$ ' in  $x\phi$  denote  $x$ : then it may denote it as a mark, as a property of significance, or it may denote what  $x$  denotes. Then we have  $x\phi$  meaning (a) that  $x$  has the property consisting in the mark ' $x$ ', or  $x(x)^0$ , (b) that  $x$  has the property of significance possessed by ' $x$ ', or  $x(x)^1$ , and (c) that  $x$  has the property that  $x$  denotes, or  $x(x)$ .

We now combine these to obtain interpretations of  $x\phi$  where ' $\phi$ ' denotes  $x\phi$ . As we have seen there will be two views possible of the structure of each interpretation. Thus we have:

- |                          |  |
|--------------------------|--|
| 5a (i) $x(x\phi)^0$      | $x$ has the property such that $x^0$ has the property $\phi^0$ . |
| (ii) $x(x)^0, x(\phi)^0$ | $x$ has the property $x^0$ , and $x$ has the property $\phi^0$ . |
| 5b (i) $x(x^1\phi^0)$    | $x$ has the property such that $x^1$ has the property $\phi^0$ . |
| (ii) $x(x)^1, x(\phi)^0$ | $x$ has the property $x^1$ , and $x$ has the property $\phi^0$ . |
| 5c (i) $x(x\phi^0)$      | $x$ has the property such that $x$ has the property $\phi^0$ .   |
| (ii) $x(x), x(\phi)^0$   | $x$ has the property $x$ , and $x$ has the property $\phi^0$ .   |
| 6a (i) $x(x^0\phi^1)$    | $x$ has the property such that $x^0$ has the property $\phi^1$ . |
| (ii) $x(x)^0, x(\phi)^1$ | $x$ has the property $x^0$ , and $x$ has the property $\phi^1$ . |

- |                          |  |
|--------------------------|--|
| 6b (i) $x(x\phi)^1$      | $x$ has the property such that $x^1$ has the property $\phi^1$ . |
| (ii) $x(x)^1, x(\phi)^1$ | $x$ has the property $x^1$ , and $x$ has the property $\phi^1$ . |
| 6c (i) $x(x\phi^1)$      | $x$ has the property such that $x$ has the property $\phi^1$ .   |
| (ii) $x(x), x(\phi)^1$   | $x$ has the property $x$ , and $x$ has the property $\phi^1$ .   |
| 7a (i) $x(x^0\phi^2)$    | $x$ has the property such that $x^0$ has the property $\phi^2$ . |
| (ii) $x(x)^0, x(\phi)^2$ | $x$ has the property $x^0$ , and $x$ has the property $\phi^2$ . |
| 7b (i) $x(x^1\phi^2)$    | $x$ has the property such that $x^1$ has the property $\phi^2$ . |
| (ii) $x(x)^1, x(\phi)^2$ | $x$ has the property $x^1$ , and $x$ has the property $\phi^2$ . |
| 7c (i) $x(x\phi^2)$      | $x$ has the property such that $x$ has the property $\phi^2$ .   |
| (ii) $x(x), x(\phi)^2$   | $x$ has the property $x$ , and $x$ has the property $\phi^2$ .   |
| 8a (i) $x(x^0\phi^3)$    | $x$ has the property such that $x^0$ has the property $\phi^3$ . |
| (ii) $x(x)^0, x(\phi)^3$ | $x$ has the property $x^0$ , and $x$ has the property $\phi^3$ . |
| 8b (i) $x(x^1\phi^3)$    | $x$ has the property such that $x^1$ has the property $\phi^3$ . |
| (ii) $x(x)^1, x(\phi)^3$ | $x$ has the property $x^1$ , and $x$ has the property $\phi^3$ . |
| 8c (i) $x(x\phi^3)$      | $x$ has the property such that $x$ has the property $\phi^3$ .   |
| (ii) $x(x), x(\phi)^3$   | $x$ has the property $x$ , and $x$ has the property $\phi^3$ .   |

We saw that the 1a-4c formulations were each capable of two interpretations : first as predications of statements, secondly as predications of the component parts of statements. 3c may be read either as a predication of a predication of an illusory subject, or as a predication of an illusory subject and a further predication of  $\phi$ . Either of these interpretations can be unendingly continued since the illusory subject refers back again to itself and whatever is predicated of it. To continue 3c (i) results in an unending sentence : to continue 3c (ii) results in an indefinite number of sentences, each saying that  $\phi$  has  $\phi$ , except the last whenever one stops, which says that the illusory subject has  $\phi$ , prompting further evaluation.

Parallel situations are found in 7c. Just as 3c was the only formulation involving both the illusory subject and the *same* interpretation of  $\phi$  at each occurrence of the symbol, so 7c is the only formulation involving the illusory property  $\phi^2$  and the *same* interpretation of the subject  $x$  for each occurrence of that symbol. Just as the first interpretation of 3c gives an unending sentence consisting of predications of predications . . . of something illusory, so 7c (i) gives an endless sentence consisting of a predication of a definite subject by an illusory property unendingly defined as that which the subject possesses: and similarly to the second interpretation of 3c, 7c (ii) gives an endless sequence of separate sentences, all saying that the property which  $x$  denotes possesses itself, except the last whenever one stops, which says that  $x$  has the illusory property—prompting further definition of that property.

The corresponding paradox from 7c (i) can be obtained by defining the property consisting in the *not* having of the illusory  $\phi^2$  and predicating it of the subject, e.g. 'x has not the property conveyed by these words', or 'x has not the property indicated here'. Then the property indicated is that of not having the property indicated: thus if  $x$  has it, it doesn't, and if it doesn't have it, it does. But in fact no property is determined either by 'x has this property' or by 'x has not this property': for the words 'this property' (or 'the property indicated here') can refer only to the meaning of the other words in the context in which they occur: there is no further entity to which they can refer: the illusion that there is gives the paradox, and is basically the same illusion as that discussed in 'this statement is false', where the words 'this statement' are thought to have some referent, other than any they can be shown to have.

We shall now consider both  $x$  and  $\phi$  as variable, letting  $x$  denote  $\phi$  while  $\phi$  denotes, first, itself, and then  $x$ : and conversely, letting  $x$  denote itself while  $\phi$  denotes again first itself, and then  $x$ .

First, then,  $x$  denotes  $\phi$  while  $\phi$  denotes itself. We have seen that there are four ways in which  $\phi$  may be thought to denote itself: thus if  $x$  denotes a self-referring  $\phi$  it must also denote it in those four ways. Thus there are sixteen possibilities of interpretation of a  $\phi x$  in which this happens, constituting the predication of  $(\phi)^0$ ,  $(\phi)^1$ ,  $(\phi)^2$  and  $(\phi)^3$  by each of those four properties.

These combinations show the structure of all possible interpretations of such a sentence as 'the last seven words in this sentence have the property these last seven words indicate', or

more briefly 'the last four words in this sentence have the property they indicate'. In this latter sentence, the first seven words correspond to  $x$ , the last four to  $\phi$ : the first seven denote the last four, and the last four refer to themselves as a property: thus  $x$  denotes  $\phi$ , and  $\phi$  denotes itself.

Consider the formulation ' $(\phi)^0$  has the property  $(\phi)^2$ ' selected from these possibilities. In this interpretation of the last word-statement given above, the first seven words ( $x$ ) denote the last four ( $\phi$ ) as marks: and the illusory property, which 'the property they indicate' is imagined to denote, is predicated of those marks. Thus the words 'the property they indicate' are being said to have a property which they indicate.

Define the property consisting in the not having of the illusory  $\phi^2$ , reconstruct the parallel word-statement accordingly, and we have (say) 'the last six words in this sentence have the property consisting in not having the property they indicate'. Then if the last six words have that property, they haven't, and if they haven't, they have. We may now make the paradox look a little more respectable by defining the property which a symbol has, when it hasn't the property it indicates, in some high-sounding term, say 'heterological', and instead of asking whether 'not having the property they indicate' has the property indicated, ask whether 'heterological' is heterological.

' $(\phi)^2$  has the property  $(\phi)^2$ ', a further selection, gives us the structure of an interpretation which requires an illusory property to have itself; paradox arises with the definition of the non-possession of  $\phi^2$  as a property and the predication of that new property of itself. Let  $p$  be the property that properties have when they do not have themselves: then if  $p$  has  $p$ ,  $p$  cannot have itself, and hence hasn't  $p$ : but if  $p$  does not have  $p$ , then it has a property such that it does not have itself, which is  $p$ : thus  $p$  has  $p$ . This is a form of Russell's paradox about classes which are not members of themselves: but as we see, the true solution is not a general judgment that to say that a class is or is not a member of itself, is meaningless, or that to say that a property has or does not have itself, is meaningless: but to show that the formulation of the paradox contains words which refer to themselves together with the illusion that what they refer to is an existent class or property. We may believe that the property of being conceivable, is conceivable, or that the class of things having been expressed in words, has been expressed in words, and define the property which properties have when they do not have themselves, and the class of classes which are not members of themselves, without paradox. But, calling

the so-defined property, and class, 'p', we cannot say, if p has p, (or if p is a member of p), p has not p (or p is not a member of p), since the deduction depends on a mis-interpretation of 'itself'. Thus; in 'p is the property which a property has when it does not have itself' what does 'itself' refer to? For the purpose of the definition of p, any property. But we are going on to say; if p has itself, then so-and-so follows. Thus for the purposes of the paradox, 'itself' must refer to the property being defined by the words among which 'itself' occurs: for the purposes of the paradox, the definition of p is 'p is the property which a property has when it does not have the property being defined'. Now it is evident that that does not define any property: the illusion that it does being basically at one with the illusion that 'this property' defines a property, and that 'what x conveys' tells us the meaning of x. However, given the illusion, the property being defined is evidently self-contradictory: for 'being defined' refers us back to the beginning of the definition, which then becomes 'p is the property which a property has when it does not have (here the definition recommences) the property which a property has when it does not have . . . ,etc., etc.'. Similarly, 'p is the class which a class belongs to when it does not belong to the class being defined' becomes 'p is the class which a class belongs to when it does not belong to (here we go back to the beginning) the class which a class belongs to when it does not belong to . . . etc. etc.'.

Next to be noted are interpretations of statements in which x denotes  $\phi$ , and  $\phi$  denotes x. An example in words is 'the property indicated by the last eight words in this sentence has the property indicated by the first eleven words'. In the nine possible interpretations which follow,  $(x)^0$ ,  $(x)^1$  and x are each predicated in turn of each of  $(\phi)^0$ ,  $(\phi)^1$  and  $\phi$ . The  $x^2$  and  $\phi^2$  illusions are not involved since no self-reference occurs.

We then have the denotation of x by itself, and of  $\phi$  by itself. Example: 'these first four words have the property indicated by these last eight words.' There are sixteen possible interpretations.

Last in this series comes the denotation of x by  $\phi$  together with that of x by itself, e.g. 'these first four words have the property indicated by themselves'. There are, again, sixteen possible interpretations.

This concludes our review of interpretations of a  $\phi x$  in which either member denotes either itself or the other.

There are, however, many further possibilities in the way of self-reference. Either member may denote, not only either

itself or its fellow singly, but also either itself or its fellow or the combination of the two. Thus while  $x$  may denote  $\phi x$ ,  $\phi$  may denote either itself or  $x$ : and while  $\phi$  denotes  $\phi x$ ,  $x$  may denote either  $\phi$  or itself. Finally,  $x$  may denote  $\phi x$  while  $\phi$  does the same. Examples in words of these respective possibilities are:

- (a)  $x$  denotes  $\phi x$ , and  $\phi$  denotes  $\phi$ —'this sentence has the property indicated by these last eight words'.
- (b)  $x$  denotes  $\phi x$ , and  $\phi$  denotes  $x$ —'this sentence has the property indicated by the first two words in it'.
- (c)  $\phi$  denotes  $\phi x$ , and  $x$  itself—'these first four words have the property indicated by this sentence'.
- (d)  $\phi$  denotes  $\phi x$ , and  $x$  denotes  $\phi$ —'the property indicated by the last six words in this sentence has the property indicated by this sentence'.
- (e)  $x$  denotes  $\phi x$ , and  $\phi$  denotes  $\phi x$ —'this sentence has the property indicated by this sentence'.

We have seen that there are twenty-four possible interpretations of a  $\phi x$  in which  $x$  denotes  $\phi x$ , and a parallel twenty-four of a  $\phi x$  in which  $\phi$  denotes  $\phi x$ . The total number of possible interpretations of (a)-(e) above would seem to be large, and some hundreds would involve the  $x^2$ ,  $\phi^2$  or derivative illusions: an account of these might reveal the structure of enigmas or contradictions not discussed above. But such a complete analysis is too long to be undertaken here.

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## VI.—IS EVERYDAY LANGUAGE INCONSISTENT ?

BY AVRUM STROLL

### I

ONE of the fundamental problems of contemporary philosophy<sup>1</sup> concerns the question of whether philosophical analysis should employ the concepts of everyday language, or whether it should employ concepts explicated in so-called formalized languages, or whether it can and should do both.<sup>2</sup> A powerful motive for adopting a purely formalist position has been the claim that everyday language is inconsistent. This claim is now made by a number of analytical philosophers. R. M. Martin, for example, says :

Now it is clear from the work of Tarski that the language of everyday speech is 'semantically closed' and hence inconsistent.<sup>3</sup>

The view that ordinary discourse is inconsistent seems naturally to suggest that in order to philosophize clearly we must first "fix up" everyday language, and that this can be done by formalizing it. Professor Martin argues for this further step as follows :

Because of the presence of the logical as well as of the semantical or epistemological antinomies, it seems very likely that natural language, even just its declarative, cognitive part, is inconsistent. For inconsistent L, the term 'analytic in L' seems scarcely worth bothering about. For inconsistent L, presumably every sentence of L is analytic. To talk about natural L with the kind of care and precision necessary for clear minded philosophy, without indicating how one would give an explicit, consistent formalization of it, is to forget the very first lessons which Frege, Carnap and Tarski have taught us.<sup>4</sup>

<sup>1</sup> For recent discussions of the problem see G. Bergmann, "Two Types of Linguistic Philosophy", *Review of Metaphysics*, March, 1952; A. Pap, "The Philosophical Analysis of Natural Language", *Methodos*, vol. 1, 1949; Benson Mates, "Analytic Sentences", *Philosophical Review*, October, 1951.

<sup>2</sup> This position has been suggested by Carl Hempel in a review of Nelson Goodman's book, "The Structure of Appearance", *Philosophical Review*, January, 1953.

<sup>3</sup> R.M. Martin, "Some Comments on Truth and Designation", *Analysis*, January, 1950, p. 65.

<sup>4</sup> Martin, "On 'Analytic'", *Philosophical Studies*, April, 1952, p. 44.

The quotations just cited may be taken as expressing two theses, which state the pure formalist case, and which we can summarize as follows :

(1) Everyday language is inconsistent.

(2) Because of (1) we have to use concepts explicated in formalized languages if we are to do philosophy in a "clear-minded way".

What I wish to show in this paper with respect to the above theses is this : We can grant that (1) is true, and at the same time plausibly hold that (2) is false. Stated otherwise, my claim is that even if we admit ordinary language to be inconsistent, the sense or senses in which it is so are not sufficiently important to induce us to give it up as a philosophical tool. If my arguments in what follows are correct, they neutralize what is, I think, one of the main reasons for adopting a purely formalist method of doing philosophical analysis.

## II

Let me begin by showing why we can admit ordinary discourse to be inconsistent without thereby conceding that we must give it up as a philosophical tool.

A common method of showing that natural languages are inconsistent is based upon Tarski's proof that semantically closed languages can be shown to be inconsistent through the development of antinomies in those languages, plus the further contention that natural languages are semantically closed. Now all such antinomies reduce to two different types. All of the sentences making up antinomies of Type "A" depend for their paradoxical nature upon the truth of an empirical sentence, which is not contained in (i.e. entailed by) the paradoxical sentence itself. I will attempt to show that paradoxes of this sort can be neutralized by a type-token distinction. Sentences of Type "B", on the other hand, do not depend for their paradoxical nature upon the truth of such an associated empirical statement. But these again can be met and rendered innocuous so far as the use of ordinary language in philosophy is concerned.

I shall begin with the consideration of a sentence of Type "A".

Suppose I enter a classroom and find written on the only blackboard in that room the following words :

*The only sentence written on this blackboard is false.*

If we call the sentence written on the blackboard "A", then

the Antinomy of the Liar can be developed when we realize that A is the only sentence written on the blackboard. It is important to notice that the antinomy can be developed when and only when we assume not only the truth or falsity of A, but also the truth of an empirical statement referring to certain circumstances about A, namely, that A is the only sentence on the blackboard. Without assuming the truth of the sentence expressing this reference, the sentence written on the blackboard cannot be shown to be self-contradictory, as the next example will show.

Suppose I enter the same classroom an hour later and find written on the only blackboard in that room the sentence "Snow is black". Let us call this sentence "B". I say, upon seeing the sentence called "B", "The only sentence written on this blackboard is false". Let us call the sentence I utter upon seeing B, "C". When I utter C in this context, C is not self-contradictory, since it refers to B. In fact, C in this context is true, since it says that B is false, and B is false. It is only when C is about C that the Antinomy of the Liar can be produced. Thus, it is necessary to distinguish between tokens of the sentence-type, "The only sentence written on this blackboard is false", for some of these tokens are self-referential and some are not. The paradox can be developed only when they are self-referential.

As the above remarks indicate, it is misleading to speak as if the sentence-type "The only sentence written on this blackboard is false", is formally self-contradictory. It is not a sentence like "Some husbands are unmarried", which can be shown to be self-contradictory merely by writing out in primitive notation the definitions of the terms making up the sentence. A contradiction can be developed from the former sentence only when it can be shown that there are occasions on which, or circumstances in which, certain tokens of the sentence-type refer to themselves. This is the additional information which is supplied by the conjoined empirical premiss. Tarski has indeed shown that if there are such occasions, and if there are such circumstances, then such tokens would be self-contradictory. But this is not the same as showing that there are in fact such occasions. In particular, it is not the same as showing that there are any such occasions in the everyday use of language.

It may be that those who assert everyday language to be inconsistent do not mean that everyday language actually uses self-referential tokens of the sort described above, but only that it might do so. And this is what I think the formalists do mean.

They mean that it is possible to construct sentence-types out of English words and according to the grammar of the English language, such that if there were circumstances in which tokens of such types were used to refer to themselves, they would be self-contradictory. Their desire is to construct languages in which even the bare possibility of such tokens occurring would be eliminated. These efforts are useful and important, and I do not wish to minimize them. For one thing, they enable us more readily to identify in a general way the features of language which give rise to paradoxes. But, at the same time, if those who urge that natural languages are inconsistent (and hence philosophically inadequate) mean merely that it is possible to construct sentences out of English words, such that if certain tokens of those sentences were used self-referentially, they would be self-contradictory, it seems to me that this result is essentially trivial, unless it can also be shown that in fact such self-referential tokens do occur in everyday speech. For if this is all that is meant when it is said that ordinary language is inconsistent, we can grant that it is inconsistent in that sense without at the same time conceding that we must give up ordinary language as a philosophical tool. Before conceding the latter point, we must be shown that natural languages are inconsistent in the stronger sense that *in fact* such self-referential sentences do or will occur in the common use of these languages. And I deny that such is the case.

The situation is similar to one in which we are told not to walk down dark streets because it is possible there are ghosts. Although we admit that it is possible there are ghosts, we continue to walk down dark streets because we deny that in fact there are any in the streets we walk down. And until this is shown, we will continue to do so.

I know no way of proving the assertion that self-referential and hence self-contradictory tokens do not or will not occur in ordinary language ; but I do not think that the assertion needs proof. It seems obviously true when one reflects on the use to which common language is put. But the point can perhaps be seen more quickly if we ask ourselves the question : Why would anybody use a self-referential expression of the paradoxical sort in ordinary life ? Why would anybody write the sentence : " The only sentence written on this blackboard is false ", on a blackboard ? What conceivable everyday use could such a sentence have ( I am, of course, assuming that the use of it by a teacher to demonstrate the Paradox of the Liar does not count as the everyday use of language ). As I indicated above, when

tokens of such a sentence are used to refer to some other sentence, then they do have a usage in ordinary speech. But there is no everyday circumstance in which such tokens are used to refer to themselves ; and this is why I say that although it is logically possible that such circumstances could exist, in fact they do not and will not. The analysis carried out by the philosopher using ordinary language in fact will not contain such self-referential sentences and hence will not be inconsistent.

It is easy to see that many of the so-called paradoxes can be developed from ordinary English sentences only by the most far-fetched distortion of the ordinary interpretations of those sentences. Consider, for instance, the famous example in which Epimenides the Cretan is reported to have said : " All Cretans are liars." Now it seems to me that considerable manipulation with the ordinary usage of language is required before we can turn Epimenides' remark into a paradox (strictly speaking, of course, this version of the paradox of the liar does not result in a contradiction, since the truth of the statement does not follow from its falsity, although its falsity follows from its truth). To begin with, it should be noted that the sentence-type " All Cretans are liars ", is not formally self-contradictory, as it stands. For if I (a non-Cretan) utter tokens of that sentence-type, those tokens may well be true about Cretans. It is only if we make some reference to the circumstances, to the situation in which the sentence-token is uttered, that it begins to appear paradoxical. This reference is to the effect that Epimenides uttered the above token, and that Epimenides himself is a Cretan. The truth of this empirical statement is necessary for the production of the paradox. But is this reference, again, sufficient to show that Epimenides' remark is paradoxical ? It is not, for it still depends on how we interpret what the words " All Cretans are liars ", mean before we can develop a paradox from them. For example, Epimenides may have meant by those words, " Every Cretan, including myself, has uttered a lie at some time ". It does not follow from the fact that Epimenides utters a sentence-token expressing the above meaning that the sentence he is now uttering is a lie. He may have lied at some other time, and now may be telling the truth. Or again, suppose Epimenides means " Each and every statement, uttered by a Cretan, except this one made by me, is a lie ". Even if this latter interpretation of his words contains a self-referential element, the self-referential element in this case does not contribute to the production of a paradox. Even if we interpret the words " All Cretans are liars ", as uttered by

Epimenides to mean : " Each and every statement asserted by every Cretan is a lie ", we cannot produce the paradox, since the truth of the statement does not follow from its falsity. If, however, we interpret Epimenides' utterance to mean : " I am lying, here, now ", then we can produce the paradox to be sure. But such an interpretation is fantastic. Why would Epimenides bother to utter a series of words having such a meaning ? In any ordinary situation we can think of, they would be pointless. But it is only if we interpret his remark in some such artificial fashion that the paradox occurs. But to produce a paradox in this way is not to find ordinary language paradoxical, for expressions having such a meaning do not and will not occur in ordinary language.

### III

The sentences of the type mentioned above are paradoxical only if we assume the truth of an empirical statement about the conditions under which they occur, *i.e.* that they are sometimes used to refer to themselves. I have argued that in ordinary language this never happens, and that if it did happen it would constitute a misuse of the language. However, it is possible to construct (say) the Antinomy of the Liar out of sentences of the natural language in such a way that the paradox cannot be neutralized by a type-token distinction. Let us consider an example of such a paradox. Suppose we have three authors, A, B, and C, who have written three books, *a*, *b*, and *c*, respectively. Now the following remark is the first sentence of *a* : " The first sentence of *b* is true." If we examine the book called "*b*" to see what it is that B says in his first sentence, we see the following sentence : " The last sentence of *c* is true." If we now examine the book called "*c*" at the reference cited, we find the following sentence : " The first sentence of *a* is false." This series of references sets up a chain, which leads to a paradox, since the first sentence of *a* presumably is both true and false. The contention of the formalists is that such a series of cross references could happen in the everyday use of language (perhaps the chain even consists of 1000 references), and therefore that everyday language is inconsistent.

But could such a chain of cross references actually take place ? The assertion that it could is plausible only if we abstract from the everyday usage of language. For in ordinary circumstances, why would A utter the sentence : " The first sentence of *b* is true ? " The answer is that he would normally utter such a

sentence only if he had seen the first sentence of B's book, and if he believed it to be true. But if he had seen the first sentence of B's book, he would not have uttered the sentence "The first sentence of *b* is true", until he had seen the last sentence of *c*; for he could not have known that the first sentence of *b* was true until he knew what it said. And therefore, he normally would look at the last sentence of *c* before saying that the first sentence of *b* is true. But the last sentence of *c* says: "The first sentence of *a* is false." But if A had seen this sentence, then why would he go on—in the first sentence of *a*—to say that what B says is true? In such a context, the remark, if seriously intended, would be pointless.

In fact, if we appeal to what would happen in ordinary circumstances, we can see that the paradox would never arise at all. The time element has been ignored. C would not say that the first sentence of *a* is false, because there would be no first sentence by A until A had seen the first sentence of *b*, plus the last sentence of *c*, since this latter sentence is what the first sentence of *b* says is true. But once he sees the last sentence of *c*, it can be seen there is no point in his uttering the first sentence of *a*, i.e. the judgment that the first sentence of *b* is true. And if so, there is no reason why C would say: "The first sentence of *a* is false", since A does not utter such a sentence. And thus the paradox never begins. The disappearance of the paradox, or something like it, invariably happens when such sentences are referred to the sorts of contexts in which they are normally used.

Paradoxes of this type, like paradoxes of Type "A", are possible only if we abstract from ordinary usage, and create artificial conditions for the occurrence of the sentences which make up the paradoxes. Thus, to say that the paradoxes show ordinary language to be inconsistent seems to me to be mistaken, or at least to be seriously misleading. It is a mistake to say that ordinary language is inconsistent if this means that the paradoxes do actually occur in ordinary language. If, however, all that is meant is that it is logically possible that such paradoxes might occur, the remark that ordinary language is inconsistent is accordingly misleading insofar as it has led some philosophers to condemn ordinary language as a philosophical instrument.

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## VII.—UNTHINKING ASSUMPTIONS AND THEIR JUSTIFICATION

By G. N. A. VESEY

THERE are certain propositions which philosophers, at one time or another, have said we 'unthinkingly assume' or 'instinctively believe'. One such proposition is that there are about us entities, 'physical objects', which can and do exist unperceived. To say that this is something we unthinkingly assume is to imply that it is a matter for dispute whether it is true. Talk of 'unthinking assumptions' is thus tied up with the idea that the philosopher's job is somehow to justify, or else show to be unjustified, what we ordinarily neither question nor think of questioning.

In this paper I shall try to show that the justification for which the philosopher searches cannot be what we ordinarily mean by 'justification'; and I shall try to do this by showing that if we use the terms 'assumption' and 'belief' in this connexion we cannot be using them as we ordinarily do. If the justification the philosopher looks for is not what we ordinarily mean by 'justification' then the products of his search cannot have the sort of significance they would have if it were.

I shall consider two questions which could be asked by the words "Are there physical objects?" The first is what I shall call 'a question *within* the realm of physical objects'. The second I shall call 'a question *about* the realm of physical objects'. I shall try to show that, in the ordinary sense of 'assumption', it would be absurd to say that in answering either of these questions in the affirmative we are making an assumption, justified or unjustified.

### *A question within the realm of physical objects*

Some philosophers have sometimes treated the words "Are there physical objects?" as if they asked a question within the realm of physical objects, that is, a question about what is in the world in the way in which "Is there a sheet of paper in the drawer?" and "Are there unicorns nowadays?" are questions about what is in the world.

For example, J. McT. E. McTaggart, in "Some Dogmas of Religion", questioned "the possibility of matter existing independently of spirit", and concluded his reflections with: "The result is that matter is in the same position as the Gorgons or

the Harpies. Its existence is a bare possibility to which it would be foolish to attach the least importance, since there is nothing to make it at all preferable to any other hypothesis."

W. T. Stace, in an article entitled 'The Refutation of Realism',<sup>1</sup> discussed the proposition that some entities sometimes exist without being experienced. He came to the conclusion: "It will follow that the realistic position that they do exist is perfectly groundless and gratuitous, and one which ought not to be believed. It will be in exactly the same position as the proposition 'there is a unicorn on the planet Mars'. I cannot prove that there is no unicorn on Mars. But since there is not the slightest reason to suppose that there is one, it is a proposition which ought not to be believed."

And G. E. Moore, in his 'Proof of an External World',<sup>2</sup> concluded, firstly, that to prove that there are objects external to our minds it is sufficient to show that there are, for example, soap-bubbles, sheets of paper, hands, shoes, and socks, and secondly, that one can show this by, for example, holding up a hand and saying "Here is a hand". It was this article which he began with a quotation from Kant's *Critique of Pure Reason*, "It still remains a scandal to philosophy . . . that the existence of things outside of us . . . must be accepted merely on faith, and that, if anyone thinks good to doubt their existence, we are unable to counter his doubts by any satisfactory proof".

The things which McTaggart, Stace, and Moore say in these passages have this in common; just as one can conceive a hand, so one can conceive Gorgons and Harpies, and even a unicorn on Mars. One can imagine the sort of circumstances which would lead astronomers or space-explorers to say "Now we know there are (or are not) unicorns on Mars". So if the words "Are there physical objects?" ask a question like the questions "Are there Gorgons or Harpies?", "Is there a unicorn on Mars?" and "Is there a hand here?" they ask a question which we know how to go about answering. In these passages McTaggart, Stace, and Moore are all treating the words "Are there physical objects?" as though they asked a question to the answering of which would be appropriate the procedures appropriate to answering a question about a member of the class of physical objects, for example, "Is there a hand here?" "Is there a unicorn on Mars?" They are all treating the words "Are there physical objects?" as though they asked a question within the realm of physical objects.

<sup>1</sup> *MIND*, 53, 1934.

<sup>2</sup> Annual Philosophical Lecture to the British Academy, 1939.

Now, if this is the question which is being asked then Moore's answer is clearly the correct one. In answering the question otherwise than as Moore does McTaggart and Stace show that they have confused it with some other question. A person cannot be consistent in saying, as Stace does, both that the proposition "Some entities sometimes exist without being experienced" is in the same position as the proposition "There is a unicorn on the planet Mars", and also that "there is no possible way in which we could know" that any single entity exists unexperienced. For there is a possible, a conceivable, way in which we could know whether there is or is not a unicorn on Mars. Nor can a person be consistent in saying, as McTaggart does, both that "matter is in the same position as the Gorgons or the Harpies" and also that "the independent existence and ultimate nature of matter is a question for metaphysics and not for science". For the question whether there are Gorgons or Harpies is a question for science, in the sense of being a question to answer which we examine the world.

If the words "There are physical objects" are to be understood as expressing an empirical proposition, like "There are cats" and "There are unicorns", then to say it is a proposition we only assume to be true implies that it is unlike these propositions in being something we believe without the sort of justification we have for believing "There are cats", or for disbelieving "There are unicorns". Saying it is a proposition we assume to be true implies that it is like "Whatever you read in the newspapers is true", which is something some people do assume. But now, if it is an empirical proposition then its truth follows from the truth of any such proposition as "There are cats", or "Here is a hand", which are things it would be absurd to say we only assume.

We know what it would be for a physical object not to exist when not perceived. If the floor of a room in which a person was sitting no longer existed when he closed his eyes, he and his chair would fall through into the room below. We can thus imagine one thing ceasing to exist when nobody is looking at it, and the havoc and amazement it would cause. In point of fact we know of nothing which does cease to exist when unperceived, except those things which are usually perceived by only one person and have no causal effects even when they are perceived, and which we call 'hallucinations'. And so the answer to the empirical question "Are there things which can exist unexperienced?" is "Yes, of course".

The categories of matter, causation and time are conceptually

linked. At any moment in time a physical object is the effect of its previous states and the cause of its subsequent ones. Consequently to ask for an answer to the question "Are there things which can exist unperceived?" which does not depend on causal evidence would be like asking for an answer to the question "How long have we been here?" which does not make use of evidence to the effect that certain things (the position of clock hands, etc.) have changed. Only the admission of such evidence gives the words "Are there things which can exist unperceived?" a meaning. If one excludes the use of such evidence it is not that one is asking a question to which there is no answer—one is not asking a question at all.

### *A question about the realm of physical objects*

In deciding what sort of procedure is appropriate to answering a question we automatically decide what sort of a question it is. If the procedure by which we answer the question "Are there physical objects?" is that by which we ordinarily answer such a question as "Is there a table in the room?" then it is a straight-forward existential question. And the answer to it is "Yes, of course", this answer to be accompanied by a puzzled frown, and the feeling that the inquirer cannot have asked what he meant to ask. If the procedure by which we answer the question is not this, then it does not ask a question about what is in the world, it does not ask a question within the realm of physical objects.

I propose now to try to give a meaning to the words "Are there physical objects?" so that they ask not a question within the realm of physical objects, but a question about the realm of physical objects. This question might more naturally be expressed by the words "Are there physical objects for us?" To explain what I mean by this question I shall introduce a new sort of statement.

The new sort of statement I shall call a 'minimum-claiming' statement. A minimum-claiming statement is the limit reached by the application of a 'claim-reducing' procedure to a 'non-minimum-claiming' statement. A non-minimum-claiming statement is such a statement as "There is a telephone in the next room". If somebody goes into the next room, comes back, and says "No, there is not", and I then say "Well, there was a ringing noise which seemed to come from there", I have reduced the claim made in my first statement. I would reduce it still further if I said, in answer to somebody's objection

that they heard no such noise "Well, it seemed to me there was a ringing noise". Ordinarily, outside of philosophical discussions, we do not make use of minimum-claiming statements, so any English sentence I might choose in which to convey a minimum-claim would be liable to misinterpretation. A person would be making a minimum-claim if he were, to use John Wisdom's phrase in his article 'Metaphysics',<sup>1</sup> "really talking only about what seems".

It would be very misleading to say that a minimum-claiming statement is about what is in one's mind, for we might also indicate that a pink elephant was hallucinatory by saying that it was only in somebody's mind, and a statement to the effect that somebody is having an hallucination is not a minimum-claiming statement. For if somebody else says that they can see what I had taken to be an hallucination this will make me doubt if I was correct in believing it to be an hallucination. Minimum-claiming statements are not corrigible in this way, whereas all non-minimum-claiming statements are corrigible.

Similarly it would be misleading to say that a minimum-claiming statement is about one's experiences. For a statement to the effect that one has a certain experience, say, as of toothache, is not the product of applying a claim-reducing procedure to a non-minimum-claiming statement. Experience statements are incorrigible, as are minimum-claiming statements, but they are not minimum-claiming statements. Nor are they non-minimum-claiming statements, for they are not corrigible. Their use is causally dependent on the use of minimum- and non-minimum-claiming statements. Thus we know how to use the word 'pain' through its being used in painful situations. All experience statements are implicitly or explicitly derivative. It is just that for some experiences, which are very important to us, we have names, such as 'pain', 'hot', 'hungry', and so on, whereas for others the derivation is more obvious, for example, "I feel as though I were a lamb in Spring".

But to say that the use of experience statements is causally dependent on the use of minimum- and non-minimum-claiming statements is not to say that they can be analysed into such statements. Their use is such as to lead us to say that the states of affairs indicated by non-minimum-claiming statements cause the states of affairs indicated by experience statements.

Both the statements "I am seeing an ashtray", when this is used to mean that I am having a veridical perception of an ashtray, and "I am having an hallucination or illusion of an

<sup>1</sup> *Proceedings Aristotelian Society*, 1950-51.

ashtray", when this is used to mean that I am having a non-veridical perception of an ashtray, are non-minimum-claiming statements. They are both corrigible. Just as I can be wrong in thinking there is an ashtray, so I can be wrong in thinking that really there is not one.

Now if the statement "It seems to me there is an ashtray", when this is understood as an incorrigible minimum-claiming statement, is true, then it logically follows that one or other of the corrigible, non-minimum-claiming statements, "I am having a veridical perception of an ashtray", and "I am having a non-veridical perception of an ashtray" is true. We have certain ways of deciding which of these exhaustive alternatives is true, certain ways of deciding whether the ashtray in our experience is a real one or not. I shall call these ways of deciding 'rules'. They differ from ordinary rules in that they are for the most part expressed not in formulae, like the rules of geometry and chess, but in our behaviour, and in the behaviour of all living creatures. The dog who, in walking across the room, avoids the table, knows the rules for deciding what is the case and what not the case. I am employing the rules when somebody asks me "How am I to know whether the thing which looks like a snake, in the corner, is one or not" and I reply "Throw something at it and see if it moves". You are employing the rules when you ask the person who has just come into the music-hall in the middle of the hypnotist's act, if he too can see the zebra sitting on the stage.

The meaning I propose for the words "Are there physical objects?" so that they ask a question about the realm of physical objects, is this: "Do we make use of these rules, the rules of the physical object language?" The answer to this question is "Yes, of course we do. There are people, mostly in mental hospitals, who suffer from the psychopathological state known as derealisation, but for most of us, most of the time, there are physical objects."

Knowing these rules is not like knowing, say, the rules of chess. It is more like knowing how to run up stairs. If one thinks hard about running up stairs—where to put one's feet, and so on—the chances are that one will, in fact, fall, just as if one thinks hard about how one is typing one is likely to make mistakes. It might be said that we 'unthinkingly know' how to run upstairs. Similarly it might be said that we unthinkingly know the rules we use in using the physical object language, that is, unthinkingly know that there are physical objects for us. But to pass from this to saying that we unthinkingly 'assume'

there are physical objects is like passing from saying we unthinkingly know how to run up stairs to saying we unthinkingly assume how to run upstairs. And this is nonsense.

### *Conclusion*

There are two dangers to be avoided in philosophical discussions of our knowledge of the physical world. One is that of identifying the familiar existential version of the question "Are there physical objects?" with the unfamiliar version I have outlined above. In the unfamiliar version it asks a question about us in a way in which the familiar version does not ask a question about us. It asks "Do we use the rules of the physical object language?" or, in other words, "Are there physical objects for us?" When the familiar and unfamiliar versions are confused the objection "But in that case if we stopped using this language, using these rules, then physical objects would cease to exist, and that is absurd" is perfectly valid, just as in the parallel objection to naïve verbalistic accounts of necessary truth, namely, "But  $2 + 2$  would still equal 4 even if nobody used ' $2 + 2$ ' and '4' interchangeably again".

But nothing in the two questions necessitates such an identification of the two questions. To see this is to see that philosophy has nothing to do with whether there are unicorns on Mars or pieces of chalk in lecture rooms.

The second danger to be avoided is that of trying to find some analogy in terms of which to understand the relationship of minimum-claiming statements and physical object statements. It has been said that to explain something is to say that it is like something else. The relationship of minimum-claiming statements and physical object statements is not like anything else. It is not like the relationship between a shadow and that of which it is a shadow, nor like that between a picture on a screen and a film in a projector, nor like that between the phenomena of electricity and the current we invent to explain them. For all these relationships are between things which are like one another in a way in which minimum-claiming statements and physical object statements are unlike one another.

Those who have talked of our unthinkingly assuming there are physical objects are those who have searched for a justification. Ordinarily when we talk about justification what we look for depends on the sort of thing we are asked to justify. Thus we justify an ethical statement in a way different from that in which we justify a mathematical one. An ethical justification is

appropriate to an ethical statement, a mathematical justification to a mathematical statement, and so on. In this article I have tried to show that it is absurd to say we assume there are physical objects, and therefore absurd to ask for a justification, as we ordinarily use that term. But the philosopher's ideas of justification is not ordinary justification. His search for justification is in effect an attempt to demonstrate an analogy which inevitably breaks down at some point. Some relationship, say that between the present content of my consciousness when I am remembering something and the thing I am remembering,<sup>1</sup> or that between individual Londoners and the average Londoner,<sup>2</sup> is accepted as being above dispute, and an attempt is made to show that, in the critical respects, there is a correspondence between the relationship between minimum-claiming statements and physical object statements, and this acceptable relationship. What is not realised is that the unique status of the different realms of being, or, in current idiom, the unique logical behaviour of the different kinds of statement, precludes such a correspondence.

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<sup>1</sup> A. C. Ewing, *Fundamental Questions of Philosophy*, pp. 92-95.

<sup>2</sup> John Wisdom's straw-man version of phenomenalism.

## VIII.—DISCUSSIONS

### A NOTE ON AN EARLY DRAFT OF LOCKE'S *ESSAY* IN THE PUBLIC RECORD OFFICE

IN the issue of *MIND* for January 1952 Mr. Laslett drew attention to the existence of a manuscript among the Shaftesbury Papers in the Public Record Office (PRO/30/24/47(8)) and conveniently termed it A(i). He described it as a variant version of Draft A of Locke's *Essay*, possibly "the first of Locke's writings on the Understanding known to be extant" (p. 90), and believed half a page of it to contain entirely new material. Having collated Draft A (as printed) with A(i) it appears to me that the latter is a copy of the former and has no new material. A(1) omits a good deal and there are mistakes of copying, but nothing fresh is added which is not found in Draft A. Some of the omissions may well be due to the fact that A(i) was made from the version of Draft A which was first written into the Commonplace book, before Locke made his corrections and additions. But to settle this finally, it would be necessary to collate A(i) with the manuscript of Draft A, and unfortunately the manuscript of Draft A is at present inaccessible.

However, a note on the document may help to make clear, first the nature of the manuscript itself, and then the relation it bears to Draft A, the earliest known version of the *Essay*.

#### I

It is an undated manuscript of about 16,425 words, written on ten folio sheets of varying sizes ( $13 \times 8\frac{1}{2}$  in.- $11\frac{1}{2} \times 7\frac{7}{8}$  in.). It is not in Locke's hand, but he endorsed it "Intellectus". It begins: "Now these Collected Ideas . . ." (Draft A § 6), and ends: ". . . Such a [power or] knowledge wch cannot" (Draft A § 44).

It is foliated, 1-20, and paginated<sup>1</sup> as follows: [1-2], 3-5, [6], 7-11, 11 repeated, 12-36. Pages 7, 9, 11 are marked also 6, 8, 10, respectively. There are no signatures. Margins, drawn freehand in ink on the left-hand side of each page, average  $\frac{1}{2}$  in.- $1\frac{1}{8}$  in. in width, and are used for paragraph numbers. There is writing on both recto and verso of each leaf (except that in the last sheet, the last three pages are blank), with an average of 45 lines to a page.

#### II

The manuscript is an incomplete copy of Draft A as published by Professor Aaron and Mr. Gibb (O.U.P., 1936) with the following

<sup>1</sup>Square brackets denote inferred numbers. The edges of the manuscript are sometimes torn.

omissions: §§ 1-5, 8, 10-12, 18, 45; most of §§ 7 and 17; half of § 30; about a third of § 33, and a few lines of §§ 42 and 44.

The fragment which, according to Mr. Laslett is "obviously nearer to those 'hasty and undigested thoughts' with which Locke tells us his whole project in fact began than anything else which has survived and may actually be a part of them" is to be found in Draft A; it is § 9 and the first part of § 13 (pp. 19-20, 25).<sup>1</sup> Moreover, for the correct ordering of the pages, the sheet containing this section comes at the beginning, and not in the middle of the draft as Mr. Laslett supposed.

If A(i) is a copy of some parts of Draft A before it was corrected, we are freed from the difficulty mentioned by Mr. Laslett on page 91 of his article, and spared the necessity of his supposition that Locke was, "at least between 1st and 10th July, 1671, both dictating in the rough on to the papers now in the Record Office, and also himself copying what he had dictated earlier into the commonplace book in the Lovelace Collection" (p. 91).

We may briefly mention the evidence for supposing (a) that A(i) is a copy made from a written document and (b) that it represents part of the original version of Draft A.

(a) Leclerc tells us in his *Life of Locke* that it was never Locke's habit to dictate, and A(i) bears signs of having been copied from a written original. There are numerous errors in transcription; e.g. misreadings ('riches' for 'virtues', p. 15; 'pope and priest' for 'past and present', p. 25; 'real' for 'natural' (nāal), p. 33); repetitions of words, and omissions of words and sentences, often with resulting reversal of meaning. Sometimes where he omits a word, the copyist leaves a space, suggesting that the omission was possibly due to a failure to read what was written. Thus, on page 36, 'Sweetness', 'Sempronia', 'birth', are omitted. Spaces are often left for Latin words (e.g. p. 44, 'etiam'; p. 50, 'vivens'; although the scribe sometimes attempts a transcript, e.g. p. 48, 'bono' for 'homo'). Evidently he was not versed in the classics, for he never attempts a Greek word either, but prudently leaves a space, as on page 48.

Omission of sentences where there is similarity of wording in two adjacent clauses is an easy error for the eye to make in copying. Thus, on page 29, Draft A has "Farther when I certainly know that such a thing did or doth exist [which I have constantly observed to produce such effects, or when I know such a thing to exist] which I have observed usually to be the effect of such a cause, can I yet certainly . . .". The sentence in brackets is omitted in A(i). Instances of similar copyist's errors occur throughout.

A(i) then is likely to be a literal transcript, imperfect, and so having verbal variations from Draft A, and from this evidence we should place Draft A first in chronology.

<sup>1</sup> The page numbers here and throughout refer to the edition of Draft A mentioned above.

We must here notice the two points listed by Mr. Laslett which lead him to place A(i) earlier than Draft A.

The first is that whereas in § 27 of Draft A (p. 41) Locke quotes the date 10<sup>o</sup> Jul 71, A(i) in the corresponding passage has 1<sup>o</sup> Jul 71.

Knowing the imperfection of the copy, and the numerous copyist's errors of several kinds already listed, we can only agree with Mr. Laslett that the possibility of a mistake in transcription must not be overlooked.

The second point, that in § 13, A(i) lists only 8 ideas where Draft A has 9 (p. 27), can be disposed of by reference to A(i). On page 3 of the manuscript the list given in Draft A is reproduced, except that where Draft A has :

"9<sup>o</sup> Of judging, i.e. guesse at the truth of words or being of things" (p. 27),

A(i) has :

"Of judging, guesse at of words or being of things" (p. 3).

(b) As to the relation between Draft A and A(i), Mr. Laslett has pointed out that A(i) is a rough working and not a revision of Draft A. It is probably an imperfectly-executed and incomplete copy of Draft A as Locke first wrote it into the Commonplace book. We know that "on rereading what he had written Locke would frequently add a further passage, which, as likely as not, would be finished many pages farther on" (Introduction, Aaron and Gibb, p. xxviii).

This may account for the fact that, apart from omissions of words and odd sentences already noted, there are six lacunae in the text of A(i). Without exception, these correspond to additions, revisions or amplifications of what Locke first wrote, and, it is significant to notice, some of them occur almost word for word in the first printed edition of the *Essay* in 1690, surely an indication that they are among Locke's most carefully thought-out passages, and therefore most likely to have been later additions.

To examine each in detail would be outside the scope of the present note, but a few observations may be made :

- (i) Part of § 7 and the whole of § 8 (Draft A, Parts of fols. 62, 63 and 87 ; pp. 16-19).

Here Locke is discussing simple ideas and their names, and the part omitted in A(i) is an elaboration of this.

- (ii) §§ 10-12 (part of fol. 63, fols. 64, 65, *in extenso*, and part of fol. 66 ; pp. 20-25).

This is a longer and more important passage where Locke begins the discussion of knowledge in general and the degrees of our knowledge which is worked out in Book 4 of the *Essay*.

The whole passage can be regarded as an interpolation between § 9 and § 13, for the former concludes with the definition of propositions as the affirmation or negation of two ideas of one another,

whilst the first sentence of § 13 follows on in sense with "The next sort of propositions is where some one or more of these simple Ideas are affirmed or denied of some substantial thing known by such a name" (p. 25).

(iii) Most of § 17, and § 18 *in extenso* (fol. 69; pp. 31-33).

§ 17 is mainly recapitulation and introduction of the next main topic, relation. Among the "relations which have fallen in by the way" (p. 31) he lists "the section of numbers and measures", etc. found in (ii) above as having been omitted from A(i). § 18, of ideas of relation, is found, in parts verbatim, in Book II. xxv. 7, 8. It is a more general discussion of relation than that which follows in § 19, found in A(i). A(i) takes up the copying again at fol. 70, after the first line and a half of fol. 69.

(iv) § 30 (fol. 76; p. 52).

This is a shorter passage, corresponding roughly to IV. iv. 4, at the end of § 30, and in the middle of fol. 76. It concerns the certainty of simple ideas received from sensation or reflection, and ends "But to returne to our purpose", which is the beginning of the following paragraph, § 31. Again, it seems likely that this is a later passage.

(v) End of § 33 (fol. 80; p. 57).

This section, coming just before the beginning of § 34, concerns the relation to probability of the two-fold testimony of other men about their experience. It is sandwiched between two sections on fol. 78, both found in A(i), and occurred on fol. 80 of Draft A. A(i) therefore follows the order as originally found in the *Commonplace book*.

(vi) § 42 (fol. 86; pp. 65, 66).

This is a short paragraph in the middle of the long § 42, and with very few variations, comprises IV. xx. 31.

The passages in Draft A corresponding to these six lacunae occur in such positions on the respective pages as to make it very possible that they were Locke's afterthoughts and were added later.

There is also (§ 27 p. 40, fol. 73, where Locke is discussing the extent of the Understanding), the omission of the sentence, "2. It hath a power to abstract." As it stands in Draft A, the passage contains mention of two capacities, both numbered 2. The first, quoted above, seems to have been an addition since it is simply "2" and not "2°" as are the other 8 numbers, and also, it continues immediately after 1° instead of beginning on a new line, as do the rest.

Of the words found in A(i) and not in Draft A we need say no more than that they are capable of explanation by the theory of the careless copyist.

It remains then for us to conclude that A(i) probably lies between the first writing of Draft A and a subsequent revision, and as such is valuable in showing which sections Locke felt it necessary to

amplify and make clearer. The accuracy of this provisional inference can be checked only by reference to the manuscript of Draft A, when it will be clear whether Locke did leave gaps in the commonplace book which he filled in after A(i) came into being. In the meantime we may simply note that there is no additional material in A(i) and that in all likelihood it is not an earlier draft of the *Essay*, but an incomplete copy of an earlier form of Draft A than the one which exists in print.

CHARLOTTE S. JOHNSTON.

## PROFESSOR RYLE'S DISCUSSION OF AGITATIONS

THE attempt to explain lawlike statements about the physical world has often led to the postulation of some sort of "substance" or "stuff", which endured through the many alterations of the world, and hence accounted for the continuity and order of those alterations. In the same way, a dispositional account of mental-concepts runs the risk of hypostatizing the patterns of behaviour, either as "Faculties" and "Ideas", or as Dispositions.

Professor Gilbert Ryle, in *Concept of Mind*,<sup>1</sup> occasionally commits this error, although he would undoubtedly repudiate it if confronted with it explicitly. One of the most interesting examples of this hypostatization is the discussion of agitations in the chapter entitled *Emotions*. An analysis of the argument will illustrate the way in which the error is committed, and the care which must be exercised to avoid objectifying dispositions, tendencies, and other pseudo-substantives.

Ryle begins the chapter by distinguishing four different kinds of "emotions": moods, motives (or inclinations), feelings, and agitations. The last three are relevant to our discussion. Motives are simply the dispositions and inclinations which he has previously analysed; pride, vanity, avarice, patriotism, laziness, and so forth. Feelings are "the sorts of things which people often describe as thrills, twinges, pangs, throbs, wrenches, itches, prickings, chills, glows, loads, qualms, hankerings, curdlings, sinkings, tensions, gnawings, and shocks (pp. 83-84)."

Quite different from these are agitations, or commotions. Agitations are conflicts or interferences between two motives, or between a motive and the world. They are frustrations, shocks, anxieties, and distractions. Ryle says:

... agitations ... presuppose the existence of inclinations which are not themselves agitations, much as eddies presuppose the existence of currents which are not themselves eddies. An eddy is an interference-condition which requires that there exist, say, two currents, or a current and a rock; an agitation requires that there exist two inclinations or an inclination and a factual impediment. (pp. 93-94.)

For example, if a man is both patriotic and cowardly, he will be torn between a desire to help his country by serving in the army, and a fear of being wounded or killed. Or, if he is avaricious, but is prevented by circumstances from acquiring a large amount of money, he will suffer anxiety and frustration. In each case, the thwarting of some motive is the essential characteristic of the agitation.

<sup>1</sup> G. Ryle, *Concept of Mind*, New York, Barnes and Noble, Inc., 1949.

The evidences of these agitations are "feelings" :

What feelings do causally belong to are agitations ; they are the signs of agitations in the same sort of way as stomach-aches are signs of indigestion. Roughly, we do not . . . act purposively because we experience feelings ; we experience feelings . . . because we are inhibited from acting purposively. (p. 106.)

And in case it is thought that these feelings are simply accidentally concomitant with the inhibitions of actions, Ryle adds : " . . . we can induce (*sic*) in ourselves genuine and acute feelings by merely imagining ourselves in agitating circumstances " (pp. 106-107).

Although this explanation of agitations may be quite perceptive, it is not available to Ryle, for it involves an essential hypostatization of motives, or inclinations, which he has earlier construed as nothing more than particular types of complex dispositions. In order to see this more clearly, let us consider the man who was described as both patriotic and cowardly. According to Ryle's theory, what does this description amount to ?

To call a man patriotic is to say that he would react to certain situations in certain ways. For example, in a discussion of the relative merits of modern nations, he would defend his own country. He would give time and energy to bond drives and safety campaigns, and would observe rationing regulations without trying to get a little extra for himself. These and many other actual and hypothetical actions form a pattern of behaviour which we label "patriotic". As Ryle observes, the man may not do all, or even most of these things, but we apply the term so long as he does, and is prone to do, a good proportion of them. The explanation of "cowardly" follows the same pattern. The cowardly man would avoid any sort of fight, shy away from physical pain, and so on. So far, there is no difficulty.

But as soon as we speak of these two motives, or inclinations, as opposing and interfering with one another, we get into trouble. For "patriotic" and "cowardly" are descriptions of the man's behaviour, and therefore, *the description of what he would do when confronted by conflicting interests must necessarily be a part of that self-same pattern.* Part of saying that this particular man is patriotic is saying that when offered a chance to serve his country, he does so *unless there is physical danger involved.* Likewise, to describe him as cowardly is to say that he shies away from danger, *although on occasion he will risk danger for the sake of his country.* Thus, his reaction to conflicting interests cannot be described in terms of the interference of two motives, for it is part of the description of both of those motives. His decision may very well induce us to change our estimate of his character, by making clearer to us the manner in which he is prone to act ; but there can be no "eddy", or "interference of currents" here.

The difficulty arises partially from a failure to realize that motive-words do not single out essentially discrete portions of a

person's behaviour, but rather are vague, general characterizations of arbitrary segments of his entire pattern of actual and hypothetical actions. His behaviour simply "is"; it grows from day to day as he lives through more and more situations, and as the sub-patterns change, our characterization of him is altered to fit the new facts.

We see, then, that feelings cannot be described as the effects of agitations, as Ryle has done. The analogy of the stream and currents is an unhappy one, for it creates the impression that there are natural lines of behaviour which constitute certain "motives", just as certain lines of water make up currents. Ryle succumbs here to the same temptations which led earlier philosophers to postulate mental and physical substances.

This also points to the fact that some notion of necessary connexions or lawlike statements is needed to complete the dispositional analysis of mental concepts. Unfortunately, Ryle does not discuss this problem, and therefore his theory seems incomplete to those philosophers who are as puzzled by counterfactual conditionals as Ryle is by "mind".

It is interesting that although scepticism with regard to physical causation can be made to seem reasonable by a Hume, not even he attempted to deny or question the validity of the concept of "personality", with the necessary connexions and continuity of emotions which it entails. Somehow it seems absurd to suggest that our feelings and actions occur haphazardly, without the slightest possibility of giving more than a "blow-by-blow" account of them as they happen. Perhaps a defence of necessary connexions would do better to start with jealousy than with billiard balls.

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## A NOTE ON LOGICAL CONNECTIVES

MR. C. H. WHITELEY in his interesting note called "The Idea of Logical Form" (MIND, October, 1951), made at least one claim that is possibly wrong, and that he should at least expatiate on and clarify. This claim seems to embody a misconception of one of the most important functions of logic with respect to either ordinary discourse or mathematical problems. On page 540 he quoted Quine's *Mathematical Logic*, page 1: "A logically true statement has this peculiarity: basic particles such as 'is', 'not', 'and' . . . etc., occur in the statement in such a way that the statement is true independently of its other ingredients." After quoting this Mr. Whiteley said that ". . . the selection of these particles is arbitrary . . .", since the "etc." leaves room for an indefinite number of other connectives or particles. Then he writes ". . . someone else may make another selection and perhaps add the word 'married' to the list". If this selection is not arbitrary, we need to know on what principle it has been made; and Mr. Whiteley intimates that there is no such principle.

Now the rôle of logic is not to give a complete list of particles or connectives, but to find as few connectives (the "primitives"), as few of these as possible: Sheffer's Stroke-function was one of the great victories of the century for logic; all the possible connectives or particles occurring essentially in arguments were by virtue of this discovery reducible by definitional rules to only one primitive. At the same time that it finds the fewest primitives possible logic must show that whatever argument one may adduce, the terms occurring essentially in it (*cf.* Quine's remark about a logically true statement) may be reduced by certain definitional rules to these few connectives or particles. When you can adduce an argument in which certain logical terms occur essentially, but cannot be reduced by definitional rules to the stroke-function, or to some combination containing only "not" and "and", then you have shown at least for these sets of primitives that the term "etc." in Quine's above-quoted statement is "arbitrary". But as long as every term occurring essentially is reducible to Sheffer's one primitive, say, we can give a rule for the selection of basic particles: any particle is basic if it may be reduced by definitional rules to a primitive like Sheffer's Stroke-function, or to a combination in which only "not" and "and" occur essentially.

Let us take the term "married" and see if it, in the essential occurrence quoted by Whiteley, reduces to primitives like "and" and "not". Whiteley's argument proceeds as follows: "Esmé is married to Evelyn, therefore either Esmé is male and Evelyn is female or Esmé is female and Evelyn is male." Now the term

"married" occurs here essentially only because it is reducible to a conjunction like the following (where "M" represents the relation of being married to, "m" represents being a male, and "f" being a female, and where "Es" stands for "Esmé" and "Ev" for "Evelyn"): Es-M-Ev; therefore both Es-m and Ev-f or both Es-f and Ev-m. Using parentheses, the argument looks like this: Es-M-Ev therefore (Es-m and Ev-f) or (Es-f and Ev-m). The connective "or" can be reduced definitionally to "not" and "and" by noting that it means "at least one, but not neither". Stated another way, the definitional rule with respect to "or" states that the expression "P or Q" is definitionally equivalent to the expression "It is not so that both not-P and not-Q". Using this definitional rule we can reduce the term "married" as it occurs in the Esmé-Evelyn argument to: not (not (Es-m and Ev-f) and not (Es-f and Ev-m)). This expression may in turn be reduced to one containing as its only logical particles Sheffer's stroke-function.

Here the logical term "married" has its logical force by virtue of the fact that it, in the context of the argument quoted by Mr. Whiteley, is reducible by definitional rules to certain primitives. This reduction to very few primitives is one of the main aims of logic. True, if one interprets all arguments in terms of say Sheffer's Stroke-function (or the "and", "not" particles) one gets some very cumbersome statements. But one always has a definite procedure for getting back to one's original argument and one has effected what Quine has called (*Methods of Logic*, New York, 1950, p. xiv) an "... essential simplification of our whole conceptual scheme ..." by having very few primitives, and simple straightforward definitional rules for constructing all possible combinations of these primitives. (In so brief a discussion as this we must ignore the rôle that rules of inference like that of *modus ponens* play in the construction of theorems, or in their proofs.)

It is true that there is an "etc." that must be attached to our lists of logical particles; and of course the word "married", in so far as it occurs in the Esmé-Evelyn sort of context, may be added to that list. But there is a rule by which we may decide whether a given term should be added to that list, and that rule states, as we have said, that the term must be reducible, in so far as it functions essentially in the argument, to the primitives of our system (be that system based on Sheffer's Stroke or the "and", "or" particles). If it is not so reducible then either the term has not occurred essentially (and this we can discover on closer examination) or our system, our choice of primitives, is at fault. But Mr. Whiteley has not presented any term occurring essentially in an argument which is at the same time irreducible to the primitives of any reputable logical system. He has therefore not shown that our selection of logical particles is arbitrary.

It may be argued that in the above remarks I have been begging the question as to whether the selection of a list of logical particles

is arbitrary. It may be said that Mr. Whiteley would agree that all terms occurring essentially (that is, all terms being used so that "if they were altered the inference might become invalid") are being used according to definitional rules, but his main point is that one can never tell in advance whether a given term one wants to call a "logical particle" will always occur essentially in all contexts. Therefore a standard list of all the logical particles cannot be drawn up; for there are no definite rules for drawing up a list of terms that always occur essentially. One may always find a logically valid statement in which the candidate for this list occurs inessentially. (Whiteley's example of an inessential occurrence of terms usually taken to be logical particles is apt here: 'I have bought some apples and some oranges, you have bought some apples and some oranges, therefore you and I have done the same sort of thing.' Here the terms "some" and "and" although usually logical particles occurring essentially happen to occur inessentially.) Moreover, as in the Esmé-Evelyn use of the term "married", one may often find some context in which a term not usually thought of as a logical particle will occur essentially.

With most of these remarks I am in agreement; but my contention is that they are not as important as Mr. Whiteley seems to think they are. Logicians have never, as far as I know, tried to draw up a list of terms that occur essentially in all possible contexts. All they have tried to do is to transform all arguments or inferences in which terms do occur essentially into statements containing only inessentially occurring terms plus a small number of primitives like "and" and "not". In other contexts "and" and "not" may not occur essentially; but this interesting fact does not trouble the formal logician: he is concerned only with particular inferences in which such terms *do* occur essentially (Mr. Whiteley emphatically believes that there are such inferences), and is concerned with the analysis of these terms in those particular instances. He is not trying to say that there are certain terms which are sacrosanct, which can never occur inessentially; nor is he trying to say that no other terms but those listed by Quine may occur essentially in a valid argument.

For these reasons I believe that Mr. Whiteley's remarks about the "arbitrariness" of the choice of basic particles are misleading. They seem to imply that it is one of the jobs of logic to draw up a complete list of "elements of meaning", to use his phrase, elements that have some quality that may be called "essentiality", a quality which they and they alone have, and which they have no matter what their contexts may be. But certainly Quine never attempted to draw up such a list, and I do not think he had such a list in mind when he mentioned the "... basic particles such as 'is', 'not', ... etc. ...". Rather I think he had in mind some of the many "topic-neutral words" to use Professor Ryle's terminology, which frequently occur essentially in logically valid arguments.

We may conclude then that his assertion that the selection of

non-primitive logical particles is arbitrary badly needs defence or clarification. The "etc." at the end of Quine's apparently desultory list is the symbol of the fact that from very few primitives many combinations may be legitimately made. In the house of Logic there are many rooms, but the key to the house is simple, and the same key seems to unlock all the rooms.

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## HEGEL AND THE SEVEN PLANETS

My reason for raking up this story is that no one in this country seems to have got it right. It also, I think, contains material of some interest to historians and philosophers of science, and to all interested in Nature and Human Nature. The reader will also understand that biographical details are merely introduced as a way of opening out the subject.

One of the first phrases I heard McTaggart utter was "I am a Hegelian". In a later book on Hegel's logic he expressed the opinion that Hegel had come nearer to the true nature of reality than any other philosopher before or since. Now all young wits in their first year enjoy nothing better than a(ny) story which reflects in any way on their elders, especially those of some repute. Here was one! "Hegelian, is he: why, that's the man who gave an *a priori* proof that there were, and could be, only seven planets." And some versions added the surprising news. "And in that very year the eighth (or maybe an eighth) planet was discovered." As I knew nothing then of the dates, I accepted the story, and as one of the would-be wits, passed it on.

In later life, having discovered that this same Hegel had a volume of 700 pages on the Philosophy of Nature, and knowing of no other philosopher who goes anything like so far to meet the naturalist, I decided to look into the matter. I found much that was weird and wonderful, and here and there some quite good empirical material, even about the planets, but nothing approaching such an *a priori* proof as I was looking for. There did not even seem to be anything which could be misunderstood or twisted into such. But there was a reference to a much earlier "Dissertation on the Orbits of the Planets", with a remark that Hegel no longer held some of the opinions there expressed. And as the Dissertation was in Latin I had to let the matter drop.

But other people still kept flogging the dead horse. Neurath of Vienna in *Erkenntnis* about 1930, repeats the story about both the *a priori* proof, and the contemporary discovery of an eighth planet. And Sarton in a quite recent book pours sarcasm on Hegel apparently believing in this alleged proof. Meanwhile, I had come across and read Lasson's German translation of the Dissertation. The translation is very free, and the work itself does not give one a very good impression of German university standards round 1800.

I shall attempt to give the reader the historical background for appreciating the point at issue. The Pythagoreans had discovered the connexion of mathematics with musical scales. Kepler and other moderns were greatly interested in such harmonies. Newton gave us his seven colours, later correlated with wave-lengths of light. Then there were intervals between the planetary orbits;

surely there must be some plan in them, some simple law or laws, in finding which we should be "thinking God's thoughts after Him". For long all efforts were baffled; then in 1772 came a ray of light, Bode's Law. Studying the not very accurate data then existing, Bode found a rule which fitted fairly well. If we call the average distance of Mercury from the Sun "a", and that of Venus "a + b", and take a = 4, and b = 3; then for the planets known in 1772 we get:—

	Bode's Law		Actual Distances : E = 10
Mercury	a	4	3.87
Venus	a + b	7	7.23
Earth	a + 2b	10	10.00
Mars	a + 4b	16	15.24
	[a + 8b]		
Jupiter	a + 16b	52	52.03
Saturn	a + 32b	100	95.39

Note that the "b"s are as  $2^0 : 2^1 : 2^2 : [2^3] : 2^4 : 2^5$ . Notice also the gap for "a + 8b", for that is the cause of all the trouble. God surely wouldn't leave a gap in such a delightful scheme. Now if we go in thought another step we shall get:

Bode's Law		Actual Distance	
a + 64b	196	191.90	Uranus (1781)

Thus Uranus, discovered after the rule, also conforms approximately. So when Hegel took up the matter there were eight terms, but only seven planets. But there were *a priori*s in those days who were not content with this. They (not Hegel) said: "There *must* be an eighth planet; not eighth in the series, but between Mars and Jupiter (i.e. 5th). Admittedly, it is quite invisible with existing instruments, and exercises no gravitational perturbation. But it must be there!"

To complete the historical sketch: two other planets have been discovered since Hegel's death; and precisely by means of the perturbations exercised (at least this is true of Neptune, 1846, if not also of Pluto, 1930).<sup>27</sup> This completes our table.

	Bode's Law		Actual Distances
Neptune	a + 128b	388	300.70
Pluto	a + 256b	772	394.60

Note that *the eighth planet* came 15 years after Hegel's death, so cannot be the one referred to in the story. And even the approximate rule no longer holds.

But we must now return to Hegel and 1801, and forget Neptune and Pluto. Some readers may be surprised that Hegel was not on the side of the *a priorists*. He accepted Uranus on the empirical evidence, and there is every reason to believe that he would have accepted, or even welcomed, Neptune, as I shall show later. But he decided to beat the *a priorists* at their own game. There is a much simpler *a priori* series of powers in Plato's *Timaeus*, stemming from the Pythagoreans.

1, 2, 3, 2<sup>2</sup>, 3<sup>2</sup>, 2<sup>3</sup>, 3<sup>3</sup> } etc. (this series need not be limited to 7 terms).  
1, 2, 3, 4, 9, 8, 27 } etc.

Now Hegel substitutes 16 for 8, and so fakes a series of 7 terms which approximately (or so he tries to show) fits the existing 7 planets. And his quite good conclusion is: "There is no need to worry about the missing eighth (or fifth) planet."

Now what was it that happened in the same year 1801, for it was not the discovery of *the eighth planet*? But something was discovered that year, on New Year's Day; a small piece of rock called Ceres, in the gap between Mars and Jupiter, "*a + 8b*". This, and many similar fragments discovered since are variously termed "asteroids", "planetoids", or even "minor planets". But they are *not* counted in the series of planets. But Hegel accepted them as closing the gap, and (for him) the argument. Note "*a + 8b*" = 28. Humboldt in his "Cosmos" gives the actual distance as 27.68.

It only remains to refer briefly to what Hegel did say about the planets in his lectures on "Philosophy of Nature". He gives (a) an inner series of 4: Mercury, Venus, Earth, Mars; (b) the asteroids Ceres, Pallas, Juno, Vesta, also 4, which happened to be all Hegel knew; (c) an outer series, Jupiter, Saturn, Uranus (and . . .?). I think Hegel would have welcomed Neptune as completing his 3rd set of 4. Sets of 12 are not unknown even in the Logic. Three, not seven, is Hegel's number; and often in the field of Nature four. It was Newton, Hegel's bugbear, who went in for seven! Whereas 12 can be made up of 3 fours or 4 threes, 7 would have to be 3 and 4 and this only gives 2 terms. Hegel would not have liked 7 in one row like the spectral colours. Even the five senses have to be triadised: (1) Touch, (2) Smell and Taste, (3) Sight and Hearing.

*To sum up*: Had Hegel any special weakness for the number 7, he could rightly have dismissed Ceres etc. as not being genuine planets. Instead he accepted without demur, the *empirical* evidence that there was something in the gap; and later gave a list of 11, including 4 planetoids. If then I have got the story right, he was not trying to prove anything, *a priori* or otherwise. He was merely demurring to the *a priori* assertion that there were 8, made before there was any empirical evidence. If, as is possible, I have not got the story right, I hope someone will correct me.

BERTRAND BEAUMONT.

## IX.—CRITICAL NOTICES

*The Language of Morals.* By R. M. HARE. Oxford: at the Clarendon Press, 1952. Pp. viii + 202. 7s. 6d.

THE importance of Mr. Hare's admirable little book is not to be judged by its size or format, or by its remarkably low price. Intended by the author to be a "clear, brief and readable introduction to ethics", ethics being taken as "the logical study of the language of morals" (p. v), it is in fact an independent investigation into the function of "prescriptive language" which carries on the exposition and discussion of the type of ethical theory which has been called "non-cognitive", "emotive-conative", "ethics without propositions". Mr. Hare has achieved his aim of brevity by leaving out, as he says, "most of those qualifications, answers to minor objections, and other defences with which the security-minded philosopher is apt to hedge himself round" (p. v). But this self-denial has not prevented him from expounding a definite ethical view in detail and with a proper cognizance of most of the objections that can be made to it. *The Language of Morals* is as philosophically serious a study of the subject as was Professor C. L. Stevenson's much longer book, and is only less important in the historical sense that, in *Ethics and Language* (1944), Stevenson ploughed ground which his predecessors in the same way of thinking (Ogden and Richards, Barnes, Carnap, Ayer) had only scratched before.

This then is a book upon which all serious students of moral philosophy—dons as well as undergraduates—will have to ponder. [Indeed I have my doubts as to whether it is altogether suitable for a "beginner" in moral philosophy as contrasted with one in what Mr. Hare calls ethics: the former should surely become acquainted with some of the things moral philosophers have said by means of the language of morals before studying the language itself.] Mr. Hare writes lucidly and with no irritating mannerisms; and, though he confines himself strictly to the study of moral language and divagates into non-linguistic moral philosophy in one passage only (pp. 140 ff., where he maintains that moral goodness is always attributed, directly or indirectly, to people), he makes it clear that he has as strong views about morality as he has about moral logic. Many people who ought to have known better have accused proponents of an "ethics without propositions" of corrupting the youth. This accusation is ridiculous enough in the case of Professor Stevenson, whose writing displays well enough his liberal utilitarianism (acquired, perhaps, during his undergraduate years at Cambridge); it would be utterly fantastic in the case of Mr. Hare, whose book shows a high earnestness reminiscent of Kant. His four

pages on moral education (pp. 74 ff.)—"How shall I bring up my children?"—form as powerful a sermon as ever his great Balliol predecessors preached in the College Chapel.

The book is divided into three parts. The first part, on the Imperative Mood, discusses the logic of the use of imperatives and modulates into ethics through a chapter on Decisions of Principle. The second part, on 'Good', makes a fresh start (and it can be read independently of the first part): it discusses the use of this word, first in non-moral, then in moral, contexts, the text for this part being the entry in the Oxford Dictionary: "Good . . . The most general adjective of commendation. . . ." Mr. Hare disentangles the evaluative from the descriptive elements in the uses of 'good' in the way Professor Stevenson has accustomed us to: he arrives at similar conclusions, but his approach is via the route by which we learn the use of good in non-moral contexts, e.g. the 'goodness' of the Oxford Sewage Farm effluent or of a shmakum (an instrument used in a fictitious ball-game). It would be invidious to compare the merits of Mr. Hare's and Professor Stevenson's treatments—both are excellent; but Mr. Hare's is certainly more suited for a "beginner". The third part of the book is concerned with 'ought' and 'right': in its concluding chapter a model language is constructed in which "artificial concepts, which could to some extent do duty for the value-words of ordinary language, are defined in terms of a modified imperative mood. This model is not to be taken too seriously" (p. 3). I found this "analytical model" very valuable in spotlighting points which had not stood out so clearly in the earlier discussion. Throughout the book Mr. Hare's concern is only with the "logic of moral terms": he nowhere lays down any moral principles, although he mentions some for purposes of illustration. The logical relations between moral judgements and imperatives are treated in detail, but Mr. Hare never discusses such questions as the relations of different moral principles to one another. He does, however, in a fine passage, say of the justification of a moral decision: "If pressed to justify a decision completely, we have to give a complete specification of the way of life of which it is a part. . . . If the inquirer still goes on asking 'But why *should* I live like that?' then there is no further answer to give him, because we have already, *ex hypothesi*, said everything that could be included in this further answer. We can only ask him to make up his mind which way he ought to live; for in the end everything rests upon such a decision of principle" (p. 69). This is Mr. Hare's moral creed; and, together with the later section in the chapter about moral education, it shows that as a moral philosopher he must be classed as a "subjectivist".

Since I am completely in agreement with Mr. Hare's "subjectivist" moral philosophy and, in his ethics, both with his insistence (as against the pure "emotivists") that *feelings* of approval are irrelevant to moral judgements and also with his method of separa-

tion of the evaluative from the descriptive elements in value-judgements, I shall devote the rest of this notice to that part of his treatment of the "language of morals" which I find less satisfying—namely, his thesis of the intimate connexion between moral judgements and commands, and the distinction of both of these from statements of fact.

Before directly discussing it, however, I must comment on Mr. Hare's criticism of the form in which this thesis has been advanced by earlier "non-cognitivists" (he mentions Carnap, Ayer and Stevenson) who, he says, hold that the function in language of imperatives (and hence of moral judgements if these are assimilated to imperatives) is "to affect causally the behaviour or emotions of the hearer" (p. 12). This account of the function of imperatives is erroneous, he thinks, because although "it is indeed true of imperative sentences that if anyone, in using them, is being sincere or honest, he intends that the person referred to should *do* something (namely, what is commanded)", yet "the processes of *telling* someone to do something, and *getting* him to do it, are quite distinct, logically, from each other" (p. 13). After we have told someone what he is to do, then, "if he is not disposed to do what we say, we may start on the wholly different process of trying to get him to do it" (p. 14). But is this a wholly different process? Surely telling a person to do something is one way of trying to get him to do it: if the telling alone does not succeed, other ways will have to be tried—rational ways like convincing him by giving reasons, irrational ways like "persuading" him with a thumb-screw. In support of his argument Mr. Hare says that an instruction to a joiner 'Supply and fit to door mortise dead latch and plastic knob furniture' is "not intended to *galvanize* [him] into activity; for such a purpose other means are employed" (p. 14). But surely galvanization of the consumer is exactly what is intended by those responsible for advertisements in the form of commands, e.g. 'Drink more beer', 'Eat more bananas'. [And I am quite unmoved by the arguments adduced by Mr. Hare in *Aristotelian Society*, Supplementary volume xxv (1951), pp. 207 ff., to show the difference between the functioning of words like 'advise', 'order', 'command', 'tell' and words like 'persuade', 'induce', 'cause', 'get'. That words in the former class can be used "performatively" whereas words in the latter class are "achievement words" may perhaps be said to show a logical distinction between the ways in which we use the words 'tell' and 'get'; but it goes no way to establish that the processes of telling and of trying to get are "wholly different".]

Mr. Hare, I think, has been led into error by thinking too much of the sentence spoken when a command is uttered and too little of the circumstances which make the hearer of the sentence regard it as a command. Why the hearer does so is because the situation is one in which he takes the speaker to be intending the command

to be obeyed, and he would not suppose the situation to be of this sort unless he had known of similar situations in which a command was obeyed. Indeed, in the section immediately preceding that which I am discussing, Mr. Hare argues against the attempt "to explain the meaning of the imperative mood in terms of wishing or any other feeling or attitude", giving as his reason the fact that "we learn how to *respond to* and use commands long before we learn the comparatively complex notions of 'wish', 'desire', 'aversion', &c." (p. 12: my italics). Here Mr. Hare is using against the "emotivist" exactly the argument I am using against his separation of commanding from getting a command obeyed.

Why Mr. Hare is so concerned to make this separation, which he supports by making what I think to be an equally illegitimate separation between telling "someone that something is the case" and "getting (or trying to get) him to believe it" (p. 13), is that he wishes to protect himself against the accusation that his "logic of moral terms" would make the function of moral judgements that of *persuading*, and hence would make it indistinguishable from that of propaganda. But Mr. Hare himself has no need for this disclaimer: the central feature in ethics for him is that of a person's individual subscription to a moral principle (which Mr. Hare treats as more analogous to a command than I shall maintain it to be); and to propagate adherence to a moral principle by one's personal example cannot be regarded as propaganda in any derogatory sense of that very emotive word.

I believe that it is this same preoccupation with sentences taken in isolation from the contexts in which they are used which is responsible for the inadequacy I find in Mr. Hare's principal thesis—that of the resemblance between commands and moral judgements and the lack of resemblance of each of these and non-moral statements of matters of fact (which I shall henceforward simply call *statements*).

Mr. Hare develops his thesis by means of a very bold device. Taking the indicative sentence 'You are going to shut the door' and the imperative sentence 'Shut the door' he "recasts" them respectively into the two sentences:

'Your shutting the door in the immediate future, yes';

'Your shutting the door in the immediate future, please.'

These two new sentences have a common part—the phrase preceding the comma—but differ in the final word. The phrase preceding the comma, which Mr. Hare calls the *phrastic (descriptor* was what he called it in his 1949 article in *MIND*, vol. lviii, pp. 21 ff.), is used for "referring" to "the thing" that both the original sentences are "about" (p. 17): it will simplify my exposition if I may call this "thing" the *proposition*, though Mr. Hare avoids using this unfashionable term, preferring instead the expression *actual or possible state of affairs* or some abbreviation of it (p. 22). The words 'yes' and 'please' following the comma in the new sentences Mr. Hare calls *neustics* (the *dictors* of his *MIND* article):

the function of the neustic is to "tell us what the speaker is saying about" the proposition referred to by the phrastic (p. 17). The neustic *yes* tells us that what the phrastic refers to is the case; the neustic *please* tells us to make what the phrastic refers to be the case. The difference between an indicative and an imperative sentence is that, when recast in what I will call Hare form, the former has *yes* as neustic and the latter *please*.

Mr. Hare never speaks of the transition from the original sentence to the sentence in Hare form (the Hare sentence, as I will call it) as a process of *translation*. He uses such verbs as 'recast', 'write', 'render', 'analyze', 'split' (into phrastic and neustic), but never 'translate'. And, of course, though he does not say so explicitly, he is wishing to do something much more than to give a rule for translating, e.g. the imperative sentence 'Make *p* the case' into the Hare sentence '*p*, please'. For he recognizes that commands may be expressed in other ways than by imperatives, and statements in other ways than by indicatives; and what he is concerned with is a comparison of the use of commands with the use of statements. I should describe what he is trying to do in the following way. He is considering two complete situations, the one being the making of a statement and including not only the words spoken but the tone of voice and other intonational features appropriate to the words being used to make a statement, the other being the giving of a command and including not only the words spoken but the intonation appropriate to the words being used to give a command; and he is rendering each into a form consisting of two verbal expressions—a phrastic followed by a neustic, the phrastic in each case representing the proposition concerned in the situation (that which is stated to be the case or ordered to be made the case), the neustic in each case indicating *everything else* in the situation which is relevant to its being the making of a statement or the giving of a command. The best comparison I can think of to rendering into Hare sentences is the representation of a musical composition by a written score, where the sequences of crochets, quavers, etc. on the score represent the sequences of relative pitches in the musical sounds, and the tempo and expression marks (e.g. *andante cantabile*) indicate how these sequences of notes are intended to be played.

Why Mr. Hare's venture is such a bold one is that to devise a notation capable, in itself, of indicating *all* the elements that are involved in a communication-situation is, I suspect, quite impossible. It would require a way of indicating separately all the diverse factors which go to make up the complete "meaning" of the original utterance in the context in which it is used, factors whose diversity and complicated inter-relationships can be ignored by no one who has studied, for example, the work of Professor I. A. Richards. Mr. Hare has therefore the exceedingly difficult task of picking out exactly those factors which are relevant for his

purpose. He is quite right, I think, in ignoring two factors—the communication by the speaker to the hearer of the proposition concerned so that, in Hare form, the phrastic will represent the same proposition to the two parties; and the expression by the speaker, and its conveyance to the hearer, of the speaker's feelings. Mr. Hare's ignoring the speaker's feelings is in accordance with his view (which I share) that the concern of both commands and moral judgements is not to convey feeling: feelings, even if they come into moral philosophy, do not enter into the logic of ethics. And he is quite right in not discussing the subtle problems involved in the communication of what is expressed by his phrastic, for these are not problems special to the language of morals. [I do not like Mr. Hare's saying that "nodding" a sentence, i.e. in Hare form using one of his two neustics *yes* or *please*, is "something that is done by anyone who uses a sentence in earnest, and does not merely mention it or quote it in inverted commas" (p. 18), since in suitable contexts I can use a sentence in earnest to put forward a proposition merely for consideration. The Hare sentence rendering this situation might be said to have the *null-neustic*, since the null-neustic will be included in all the other neustics—on the assumption that the phrastic is always assumed to be understood in the same sense by all parties concerned.]

So far, so good. But Mr. Hare's limiting himself to the two neustics *yes* and *please* does not, I think, do justice to the varieties of *intention* that are present in the various statement, command and moral judgement situations. He speaks throughout as if an indicative is always used "for telling someone that something is the case", an imperative "for telling someone to make something the case" (p. 5), i.e. as if only two sorts of intentions on the part of the speaker need to be distinguished. Since I believe that Mr. Hare's *simplisme* about intentions is responsible for his making the most important use of a moral judgement—that of subscribing to a moral principle—more analogous to that of a command than I take it to be, I shall develop this criticism at some length. In doing so I shall emulate Mr. Hare's boldness by using his method of renderings into phrastics and neustics: I shall require to distinguish nine types of situation and shall therefore need nine neustics. It is perhaps unnecessary to tell readers of *MIND* that what follows is as much a development as a criticism of Mr. Hare's profitable as well as bold way of tackling his subject: in matters of notational technique, imitation is the sincerest form of flattery.

I will begin with statements, where I shall distinguish three types of use. The first type (call it a *communication of information*) occurs when I say the sentence with the intention both of telling you what to believe and of letting you know that I believe it myself. This is the most usual type of use of statement: it occurs in all primary school teaching. In Mr. Hare's language (p. 19) I am both "affirming" the sentence and intending you to "assent to" it.

In this situation my sentence can be rendered as a Hare sentence with a neustic which I will write as *credo-crede*. [No significance is to be attached to the etymological fact that, for convenience, I construct my neustics out of Latin verbs: my nine neustics are to be regarded grammatically as on a par with Mr. Hare's *yes* and *please*.] My communication of information will succeed if you do what I intend you to do, i.e. believe the proposition as being a proposition which I believe.

The second type of use of statement (call it a *confession of belief*) occurs when I say the sentence with the intention of letting you know what I believe but without the intention of telling you to believe it. This use is, I take it, the proper academic teaching use: this whole critical notice is an instance of it. The context of the notice (i.e. its appearing in MIND) shows that, while I am telling you what I think about Mr. Hare's book, I do not intend that you should believe what I am saying merely because I say it. In Mr. Hare's language, I am "affirming" the sentences in this notice but not intending you to "assent to" them. My sentences are renderable as Hare sentences with what I will call the *credo* neustic. My confession of belief to you will succeed if you believe that I believe the propositions: it will fail if you mistake my intention and believe the propositions for the reason that I intend you to believe them.

The third type of use of statement (call it an *instruction to believe*) occurs when I say a sentence with the intention of telling you what to believe but without the intention of letting you know that I believe it myself. To avoid risk of misunderstanding I must then use a sentence which is renderable as a Hare sentence with what I will call the *crede* neustic. This would show that I intended you to "assent to" the sentence without supposing that I "affirm" it. Very few ordinary statement uses are of this type: an example would be a statement by an examination crammer prefaced with the remark "Of course I myself don't believe what I am going to tell you, but the examiners believe it; so I want you to". However, there are, alas, many situations to which a Hare sentence with neustic *crede* would be proper but in which a sentence is in fact used in such a way as to be renderable as a Hare sentence with neustic *credo-crede*: when the speaker of such a sentence not only does not believe the proposition but positively disbelieves it, he is speaking with intent to deceive, and, if the proposition is in fact false, he is telling a lie.

It is worth remarking that to use the Hare sentence 'My believing that it will freeze tonight, *crede*' is (in a certain sense) equivalent to the use of the Hare sentence 'Its freezing tonight, *credo*'. The "parenthetical" use of 'I believe' as in 'It will freeze tonight, I believe' is renderable as a Hare sentence with phrastic 'Its freezing tonight' and either with neustic *credo-crede* or with neustic *credo* according to the context of the parenthetical use.

Let us now turn to the use of commands (commands proper, i.e. second-person commands). Here in contrast with the case of statements, where it was necessary to distinguish three types of use having, in Hare form, three different neustics (one being the combination of the other two), there is, I think, a unique type of use having, in Hare form, a neustic which I will write as *fac*, e.g. the command 'Peel the potatoes' is, in Hare form, 'Your peeling the potatoes, *fac*'. This use occurs when I say a sentence with the intention of telling you what to do, and it corresponds to the statement use which, in Hare form, has *crede* as neustic. Why it is unnecessary to discuss command uses corresponding to the statement uses having the neustics *credo* and *credo-crede* is that the intention expressed in a command is never (it might be safer to say hardly ever) that of letting the hearer know anything about "the speaker's frame of mind". As Mr. Hare says, "a statement is held to be sincere only if the speaker believes it" (I should here qualify statement by adding as used in my first or second type of use), whereas the criterion for sincerity in the use of imperative sentences is that the speaker "intends that the person referred to should *do* something" (p. 13). And we are not concerned with complications arising from the hearer's mistaking the intention of the speaker, just as we are ignoring complications arising out of the hearer's misunderstanding the phrastic. Mr. Hare (pp. 18 f.) seems to me wrong in supposing that there is much in common between "nodding" or "affirming" a command and "nodding" or "affirming" a statement in my first or second type of use. It is only in my third statement use that the sense of "nodding" or "affirming" the statement is similar to that of "nodding" or "affirming" a command—namely, that of saying the sentence with a certain intention as to what the hearer should do or believe. This is because a statement, in my third type of use, is a command—a command to believe.

There is, however, one type of use of second-person commands (call it a *request for assistance*) in which the command could be said to be affirmed by the speaker in a similar way to that in which the speaker affirms a statement of my first or second type. If I give the command 'Help me peel the potatoes' in a situation in which I am starting to peel them, I say the sentence with the intention both of telling you to peel potatoes and of letting you know that I am going to peel potatoes myself. In Hare form the request for assistance is 'Our both peeling the potatoes, *facio-fac*'. This is analogous to the *credo-crede* neustic in my first type of statement use. And there is one type of use of sentence (call it an *expression of resolve*) which, in Hare form, has the neustic *facio*—namely, when I am starting to peel the potatoes and say 'I will peel the potatoes myself'. [The difference in the English idioms between 'I will peel the potatoes' and 'I shall peel the potatoes' is essentially the difference between a Hare sentence whose phrastic is

'My peeling the potatoes' and whose neustic is *facio* and a Hare sentence with the same phrastic and whose neustic is one of the three statement neustics *credo*, *crede* or *credo-crede*.]

Mr. Hare regards resolves (rendered by my *facio* neustic) as "closely similar to" first-person commands (p. 20). But the only sense which I can give to the notion of first-person command is that of one part of myself commanding another part to do something. In this case the expression by a sentence of a first-person command will tell you (a third party to the command situation) only that one part of myself is telling another part to do something and not that that other part is doing, or even resolving to do, that something. In resolves, as Mr. Hare says, "affirmation and assent are identical" (p. 20); but he is wrong in supposing that this is also the case in first-person commands, where affirmation by the commanding part of myself is distinct from assent by the commanded part. So I shall not regard a resolve as any sort of command, though I am prepared to class them, along with commands proper and requests for assistance, under Mr. Hare's generic term *prescription*.

We are now in a position to consider uses of sentences containing the word 'ought'. Since I agree with Mr. Hare that in moral uses of 'ought' there is always reference to a universal moral principle, I shall confine my attention to uses of expressions of such principles (which I shall call uses of *moral maxims*), e.g. 'Lies ought never to be told', and ignore the minor complications that will be required to deal with particular 'ought'-sentences, e.g. 'You ought not to lie (or to have lied) to Mr. Smith at noon on Christmas Day 1953'.

The essential thesis of a "non-cognitive" theory of ethics is to maintain that the specifically moral sense of 'ought' is not descriptive, so that such 'ought'-sentences are not used to express propositions *about* obligations. This does not imply that they do not express propositions at all, only that the propositions concerned are non-moral propositions. ["Ethics without propositions" is to be construed as ethics without moral propositions.] Mr. Hare would take the proposition expressed in the moral maxim 'Lies ought never to be told' to be the proposition that no one ever has told a lie in the past, ever is lying now or ever will lie in the future. Accordingly he would render the maxim as a Hare sentence having as its phrastic 'No lies being told by anyone ever'. I entirely agree with Mr. Hare in regarding the proposition expressed by this phrastic as being the relevant one, but I am not satisfied with treating the use of the maxim as being such that it is renderable as a Hare sentence with this phrastic and with one of the prescriptive neustics—Mr. Hare's *please* or my *facio-fac* or *facio* or *fac*.

This is because a resolve of mine, or a command given by me to you, to make it the case that no lies ever have been told, ever are being told or ever will be told is one impossible of fulfilment or

obedience—logically impossible, so far as concerns lies which have been told in the past ; causally impossible, so far as the lies being told now or to be told in the future are outside my power, or your power, to prevent. What, following Mr. Hare, I will refer to as my *subscribing* to a moral principle is, I think, expressible by such a sentence as 'I will play my part in seeing that no lies are ever told' or 'I will ensure, so far as it rests with me, that no lies are ever told'. This is renderable as a Hare sentence with what I will call the *facio-quoad-possum* neustic. Similarly an injunction to you to play your part in securing that no lies are ever told will be renderable as a Hare sentence with the *fac-quoad-potes* neustic. And a sentence used to convey both my subscription to the principle and this injunction to you will be renderable as a Hare sentence with the neustic *facio-quoad-possum-fac-quoad-potes*. All these three cases are expressible, in different contexts, by a use of the sentence 'Lies ought never to be told': the context will show whether this moral maxim 'Lies ought never to be told' is to be taken as a subscription to a moral principle (a *moral subscription*), as an injunction to act in accordance with a moral principle (a *moral injunction*) or as both at once (a *request for moral co-operation*).

In order to discuss how far the uses of moral maxims compare and contrast with uses of statements and prescriptions, let us arrange them in the following table, where the neustics in the renderings in Hare form are put in brackets :

<i>Statements</i>	<i>Prescriptions</i>	<i>Moral maxims</i>
Confession of belief ( <i>credo</i> )	Expression of resolve ( <i>facio</i> )	Moral subscription ( <i>facio-quoad-possum</i> )
Instruction to believe ( <i>crede</i> )	Command proper ( <i>fac</i> )	Moral injunction ( <i>fac-quoad-potes</i> )
Communication of information ( <i>credo-crede</i> )	Request for assistance ( <i>facio-fac</i> )	Request for moral co-operation ( <i>facio-quoad-possum- fac-quoad-potes</i> )

In contrast with this ninefold classification, corresponding to the occurrence of nine neustics in the Hare sentences, Mr. Hare's classification by means of his two neustics *yes* and *please* yields the dichotomy :

Statements (*yes*)

Prescriptions (*please*) ;

and for him moral maxims fall into the prescription class. Though it would be inaccurate to call Mr. Hare without qualification an "imperativist" in ethics, since his moral prescriptions differ from imperatives in the important respect of being "properly universal" (pp. 175 ff.), it would be correct to call him an ethical "prescriptionist".

One of the reasons why imperativism in ethics has seemed to

many philosophers to give an inadequate account of the use of moral maxims is that, when a moral philosopher (here contrasted with a teacher of morality) uses the sentence 'Lies ought never to be told', it is impossible to maintain that he is giving any command, in any reasonably extended sense of command, and much more plausible to maintain that he is making a statement, albeit of a peculiar character. Mr. Hare will appear to be referring to this non-commanding use when he proposes as an analysis of what is expressed by the sentence 'You ought to tell him the truth' what would be expressed by the sentence 'If you do not tell him the truth, you will be breaking a general "ought"-principle to which I hereby subscribe' (p. 191). This use of a moral maxim, which I have called *moral subscription*, appears in my ninefold classification as analogous, not to any form of command, but to an expression of resolve and to a confession of belief. The fact that this use of a moral maxim is the one in which philosophers have been particularly interested would be explained by the fact that the understanding of a moral injunction (or of a command proper) presupposes an understanding of a moral subscription (or of an expression of resolve) in just the same way as understanding an instruction to believe presupposes an understanding of a confession of belief. Just as I cannot tell anyone to believe something without knowing what it is to believe it myself, so I cannot tell anyone to do something (or to play his part in doing something) without knowing what it is to do it myself (or to play my part in doing it).

The philosophically less important use of a moral maxim—as a *moral injunction*—will, of course, be analogous to a command proper; so also will be the instruction-to believe use of a statement. And a request for moral co-operation will, like a communication of information, have an imperative element in it. But if we take the philosophically fundamental use of moral maxims to be that of moral subscription, this use will be classed, not with any form of command or injunction, but along with confessions of belief and expressions of resolve in what may be called the class of *declarations*.

This analysis suggests that it is not helpful to treat this fundamental, declaratory use of moral maxims as resembling in a philosophically important respect that of imperatives. What moral subscriptions resemble are expressions of resolve; so much so that they are both naturally called *resolutions*. The only thing that could be called an imperative element in a resolution is that, in uttering it, the speaker intends to let the hearer know that he (the speaker) will do something or will play his part in doing something; and this imperative element also occurs in the use of a confession of belief. A fully imperative element—that of telling the hearer to do something or to play his part in doing something—only occurs in the less fundamental uses of moral maxims, i.e. in moral injunctions and in requests for moral co-operation; but a similar fully imperative element—that of telling the hearer to believe something—occurs

in the normal, communication-of-information use of statements. Philosophers who have criticized imperativism in ethics have been right in insisting that, though the moral maxim 'Lies ought never to be told' may be used as an injunction or a request for co-operation (and though they may admit that it is usually used in one of these two ways), yet its fundamental use is declaratory rather than directive. Supporters of an "ethics without propositions" only weaken their case by attempting to construe moral maxims as concealed imperatives.

I am not sure how much Mr. Hare would disagree with me about this. As I have said, he is a "prescriptionist" rather than an "imperativist" in ethics. He only speaks of first-person commands ('Let me do so and so') in three places—page 20, which I have already commented upon; page 168, where his argument could equally well be put in terms of resolutions; and page 187, where, after mentioning other forms of the imperative mood, he says that "there is also a form 'Let me . . .' which serves as a first-person singular imperative" and goes on to say that this "person" is "of [a] different form, in English, from the second person, and may be of a somewhat different logical character" (my italics). The term he uses to cover moral and other evaluative language as well as imperatives is always *prescriptive language*; and, as I have said, I have no objection to classing an expression of resolve as a prescription: I can prescribe for myself a course of action, although I cannot, without splitting of personality, command myself to do it. And just after the eloquent confession of "subjectivist" faith to which I have already referred Mr. Hare emphasizes the Kantian character of his doctrine: "We have to make our own decisions of [moral] principle. Other people cannot make them for us unless we have first decided to take their advice or obey their orders" (p. 70). It is clear that Mr. Hare takes these personal decisions, expressed not by any form of command proper but by subscription to the moral principle concerned, as the fundamental data of ethics. Here he would appear to agree with what I have been saying. Nevertheless, by subsuming subscriptions under prescriptions and also regarding all prescriptions as having an imperative element, he is making it appear that the use of a command rather than that of a resolution is the central feature in the "language of morals". At the beginning of his book he writes: "Although it is no part of my purpose to 'reduce' moral language to imperatives, the study of imperatives is by far the best introduction to the study of ethics" (p. 2); and this remark sets the tone of his exposition. And his restricting himself to the two neustics *yes* and *please* in his rendering of relevant uses of descriptive and of prescriptive language ignores distinctions between different uses of descriptions and, by conflating resolutions with commands, conceals the fundamental use of a maxim expressing a moral principle—namely, to prescribe for myself a course of action.

It may, however, be said that, in concentrating on Mr. Hare's failure to make a delicate distinction between resolutions and commands, I have entirely missed the main point of his twofold classification of neustics, this being to distinguish between the use of an indicative sentence "for telling someone that something is the case" and the use of an imperative sentence "for telling someone to make something the case" (p. 5). In terms of my ninefold classification of neustics, it may be said that Mr. Hare is primarily concerned with the distinction between those in my first column which convey something about *belief* and those in my other two columns which convey something about *action*. "The reason why actions are in a peculiar way revelatory of moral principles is that the function of moral principles is to guide conduct" (p. 1); and Mr. Hare's primary concern, it may be said, is to contrast the use of prescriptive language in guiding conduct with the use of descriptive language in guiding belief, such subtleties as the distinction between guiding myself and guiding another being of minor importance.

This defence for Mr. Hare's treatment would be cogent if he had anywhere discussed the relation between telling one "what is going to happen or be the case" (i.e. guiding belief) and telling one "to make it the case" (i.e. guiding action) (p. 17). But he never seriously considers the function of descriptive language: though he discusses in detail the way in which prescriptive language is not descriptive, there are only a few sentences in the book (on pp. 46, 59, 163) in which he refers to the possibility of considering descriptive language as being, in its own way, prescriptive and "action-guiding". I do not blame Mr. Hare for having avoided getting involved in the analysis of belief and of its relation to action: he has deliberately confined himself to a study of language—the "language of morals"—and problems of belief are not, I think, best studied in linguistic terms. But he cannot be said to have shown that "actions are in a peculiar way revelatory of moral principles" (p. 1: my italics) when he has made no attempt to argue the thesis that, unlike moral judgements, beliefs in non-moral propositions ("plain judgements of fact") do *not* have as their function (or as an important part of their function) that of guiding conduct.

The very first sentence of Mr. Hare's book—"If we were to ask of a person 'What are his moral principles?' the way in which we could be most sure of a true answer would be by studying what he *did*" (p. 1)—immediately suggested to me, as it would to any philosopher attracted to a behaviourist or pragmatist view of belief: "Hare's remark is true. But would it not have been equally true if he had written 'beliefs' instead of 'moral principles'?" The Bainian theory of belief, as consisting in a disposition ("preparedness") to act, under suitable circumstances, in ways which are appropriate to the truth of the proposition, may

will be criticized for attaching no importance to feelings of conviction, expectation, surprise, etc. But Mr. Hare is equally behaviouristic in attaching (quite rightly, in my opinion) no importance to feelings, whether of "obligation" or of "approval", in his discussion of moral principles. If we ignore feelings of all sorts an analysis of 'I believe *p*' into 'I am acting in such a way that my actions are appropriate to *p*'s being true' has an obvious resemblance to an analysis of 'I subscribe to *q*' into 'I am acting in such a way as to play my part in making *q* true'. There are, of course, obvious differences: in the case of belief, the truth of *p* is, as it were, the Pole Star to which I orient my behaviour; in the case of moral subscription, the truth of *q* is more like the Rainbow's End whose orientation depends, in part, upon my behaviour. But both Pole Star and Rainbow's End can guide my footsteps. Now that Professor Stevenson and Mr. Hare have done such distinguished service in clarifying our understanding of how we use our ethical language, the way seems open for a frontal attack upon the problem of the relation between the manner in which our moral judgements are bound up with our actions and the manner in which our "plain judgements of fact" are bound up with our actions; and this is what I take to be the crucial problem, at the present moment, before those of us who are concerned to propound and to defend a theory of "ethics without propositions".

R. B. BRAITHWAITE.

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*What is Value?*—An Essay in Philosophical Analysis. By EVERETT W. HALL. Routledge and Kegan Paul Ltd. Pp. xiii + 255. 25s.

It is as welcome as it is rare to find an ethical writer who, while adopting unreservedly the standpoint of those who call themselves objectivists, is prepared to defend this standpoint by the method of linguistic analysis. Professor Hall's book is a running commentary upon the progress of his soul from the crude 'material-mode' question 'What is Value?' to the subtler and more fruitful questions 'What is the proper syntax of value-sentences?' and 'What is the significance of linguistic analysis for value-theory?'. Both the progress and the commentary on it are tortuous and hesitating—"I have found it necessary", he writes, 'to use the ideas and expressions of others as approximations, needing qualification to indicate the sort of view that I am trying to formulate clearly to myself and communicate understandably to others'. The 'ideas and expressions of others' suffer some mangling in the process (thus he attributes to Professor Ayer on page 68, to myself on page 142 n., and to Mr. Strawson on page 228 n. confusions of which our original texts are innocent); but it would be unkind to

demand either directness of argument or accuracy of exegesis from a writer who is obviously more than fully occupied in wrestling with the inherent difficulties of the subject.

He starts from the presupposition that 'there is value and it is unique'. He calls this 'the assumption of the objectivity of values' (pp. 1, 2 n.). He therefore thinks it necessary to give such an account of the nature of values as will leave them 'in the world' and yet keep them distinct from (though 'co-ordinate with') another sort of thing that is to be found in the world, 'facts'. 'Our problem', he says on page 113, 'is to have value in the world, not merely in our language, but to have it there in some other rôle than that of a property, whether a property of particulars or of facts. Value is to be somehow co-ordinate with fact, and thus to have a status similar to fact.'

He thinks that he can show that value is 'in the world' if he can show by logical analysis that value-sentences are not reducible to any other sort of sentences, and in particular not to factual sentences. He seeks the source of this irreducibility (rightly as I think) in that feature of value-sentences—whatever we are to call it—which they share with imperatives. He is thus led to devote much space to a very interesting discussion of the logic of these latter. He has little difficulty in disposing of those theories (such as Bohnert's) which would reduce imperatives to indicatives. But in his main argument he goes further than this. He thinks that in order to show that imperatives are not reducible to indicatives (and thus, according to his programme, that value is different from fact) he has to show that the 'syntax' of imperatives is *completely* different from that of indicatives. This involves showing that 'not' and all the common logical connectives have a different behaviour in imperatives from that which they have in indicatives. Any theory which, while agreeing that imperatives are not reducible to indicatives, nevertheless holds that, so far as the logical connectives go, their logic is the same, is dismissed by Professor Hall as a 'trivialisation' of imperative logic (p. 141). It is not clear why he thinks this pejorative term appropriate; imperative logic is not trivialised by showing it to be similar to indicative logic, unless indicative logic also is held to be trivial; but what would it be like to hold this?

The most fundamental difference between the logics of imperatives and indicatives which he claims to observe (pp. 125 ff.) is that there are two ways of negating an imperative sentence, one of which has no counterpart in the case of indicatives. Thus the command 'Donald, wear your rubbers!' <sup>1</sup> may be negated in the ordinary

<sup>1</sup> He follows the queer practice, which has become quite common, of putting an exclamation mark after those imperatives which he is *mentioning*, while omitting it (correctly) after imperatives which he is *using*. Although I agree with Fowler's judgment (*Modern English Usage*, s.v. 'Stops: Exclamation') I will in this review err with Professor Hall.

way by saying 'Donald, do not wear your rubbers!'; but it may also be 'cancelled' by saying 'Donald, you don't have to wear yours rubbers!' Thus instead of a pair of sentences, analogous to the indicative pair 'Donald is wearing his rubbers' and 'Donald is not wearing his rubbers',<sup>1</sup> we have a quartet, which he writes as follows, altering the original two commands in a way which is not above suspicion :

Donald, you have to wear your rubbers !

Donald, you don't have to wear your rubbers !

Donald, you have to refrain from wearing your rubbers !

Donald, you don't have to refrain from wearing your rubbers !

This difference in regard to negation gives rise, as he thinks, to differences in the behaviour of other logical words.

So far he has not laid great emphasis on the difference between imperatives proper and 'normatives' (as examples of 'normatives' he gives 'I ought to go', 'It would be a good thing if Iowa lost this game' and 'The right thing to do is to keep your promise'). He now turns to the relation between these two sorts of sentence. He thinks that normatives cannot, as has sometimes been suggested, be reduced to imperatives (because, he says, normatives can be in the past tense and in all persons, but imperatives can only be in the future tense, and their apparent occurrences in other persons than the second can all be reduced to second-person imperatives). But he makes the suggestion that ordinary imperatives can be reduced to normatives. Thus 'Give me that book!' can, he says, be replaced by 'You ought to give me that book' (p. 158). And so he thinks that what he has said about the peculiar logic of imperatives can be applied without alteration to normatives. He then seeks to complete his analysis of value-sentences by showing that they can all be reduced to normatives, and specifically to 'ought'-sentences. This leads him into an interesting, though involved, discussion of the rival views on this question of Moore and Prichard and their supporters.

He concludes this part of his book with an attempt to draw an analogy between 'legitimate' as predicated of normative sentences and 'true' as predicated of descriptive sentences (pp. 187 ff.). In accord with his view about the two ways in which normative sentences can be negated, he thinks that they can have three 'legitimacy-values'. These are arrived at as follows. He writes the normative form of the imperative quartet given above thus :

Donald ought to wear his rubbers.

There is no ought about Donald's wearing his rubbers.

Donald ought to refrain from wearing his rubbers.

There is no ought about Donald's refraining from wearing his rubbers.

<sup>1</sup> He sometimes puts the indicative which corresponds to an imperative into the present, sometimes into the future tense (pp. 125, 148). The future is surely correct.

He then says that the second and fourth of these can be combined into a 'single, simple, normative' (but why 'simple'—for the two are by no means equivalent?). If this latter holds, then 'Donald ought to wear his rubbers' fails to have legitimacy in one way; but if the third of the four sentences holds, it fails in another. Thus, whereas indicative sentences can normally have only two values (true and false), imperatives and normatives can have three.

I hope that the above summary of Professor Hall's views is not utterly unjust. Are they correct? Many would agree with him that value-judgments are radically different from statements of fact. But it is not clear to me how, in any usual sense of the word, such a view is consistent with 'objectivism'. It is true that the word is so ambiguous that there are very few ethical theories which cannot claim to be in some sense 'objectivist'. But would any of the commoner sorts of objectivist be satisfied with any ethical theory which did not hold that value-judgments *were* statements of fact?

Nor is the argument beyond reproach whereby he seeks to prove that imperatives are not reducible to indicatives. He thinks that he can fulfil a necessary and sufficient condition of such a proof if he can show that 'not' and the common logical connectives behave differently in the two moods. I shall try to make clear (1) that this is not a necessary condition; (2) that it is not a sufficient condition; (3) that Professor Hall does not fulfil it.

(1) He is clearly right in holding that, for example, the pair of commands 'Wear your rubbers!' and 'Do not wear your rubbers!' is not reducible to the pair of statements 'You are going to wear your rubbers' and 'You are not going to wear your rubbers' (or for that matter to any other pair of statements). But if we are asked to prove this, we can do it by showing that *those features of the first pair of sentences which make us call them imperatives* are not found in the second pair, and vice versa. It is not necessary to show that *no* feature occurs in the first pair which occurs in the second and vice versa. In particular, it is not necessary to show that 'not' behaves differently, any more than it is necessary to show that 'rubbers' behaves differently. And therefore, in order to show that imperatives are not reducible to indicatives, it is not necessary to prove what Professor Hall seeks to prove.

(2) But it is not *sufficient* either. For even if there were a class of sentences in which 'not' and the logical connectives behaved differently from similar words in the commonest kind of indicative sentences, this would not prove that this class of sentences is not indicative—it might be a class of indicatives of a peculiar sort, distinguished from the ordinary sort by this very feature. Let us suppose (without committing ourselves either to the truth of the doctrine or to the utility of this way of expounding it) that there is a class of sentences in ordinary language (or could be in an artificial language) whose logic is 'multi-valued', and in which,

therefore, the word 'not', and any connectives which are related to it, have a different use or meaning from that which they have in a two-valued logic. This in itself would not show that these sentences were not indicatives; and therefore to show that *imperatives* have these features would not be to show that *they* are not indicatives.

But we may go further than this. It is possible, using methods analogous to Professor Hall's, to construct quartets of *indicative* sentences which have the same feature as his quartet of imperative sentences; and this shows fairly conclusively that he has not hit upon a distinguishing feature of imperatives. His quartet is generated from the pair 'Donald, wear your rubbers!' and 'Donald, do not wear your rubbers!' by 'cancelling' each of them. The first, when 'cancelled', he writes 'Donald, you don't have to wear your rubbers!'; and the second, when 'cancelled', he writes 'Donald, you don't have to refrain from wearing your rubbers!'. He then rewrites the original two sentences in a way which looks more homogeneous with these latter two; he writes them 'Donald, you have to wear your rubbers!' and 'Donald, you have to refrain from wearing your rubbers!'. But the quartet which is thus produced can be written more idiomatically as follows:

Donald, you must wear your rubbers.

Donald, you may refrain from wearing your rubbers.

Donald, you must not wear your rubbers (must refrain from wearing your rubbers).

Donald, you may wear your rubbers.

In fact, they might all be described as *modal* sentences of an imperative sort, and none of them—either in Professor Hall's version or in mine—is an ordinary imperative.

But if we are allowed to pass from non-modal sentences to modal ones, much the same thing can be done in the indicative mood by using another sense of 'may' and 'must'. Instead of the pair 'Donald, you are going to wear your rubbers' and 'Donald, you are not going to wear your rubbers', we have the quartet:

Donald, you must be going to wear your rubbers.

Donald, you may be going to refrain from wearing your rubbers.

Donald, you must be going to refrain from wearing your rubbers.

Donald, you may be going to wear your rubbers.

'May' in one sense can be used to 'cancel' an indicative, just as in another it can be used to 'cancel' an imperative. And 'must' in both senses 'cancels' the 'cancellation', thereby restoring something stronger than the original statement or command. Once we stray from plain non-modal sentences into any of the many kinds of modal sentences, this sort of thing happens to negation; but it happens in both moods alike.

(3) There is not space to deal with all the examples whereby Professor Hall and those on whom he draws have sought to show

that 'not', etc. behave differently in the two moods. I shall take those examples which seem the most plausible. Note, that I am not here claiming positively that the two moods are alike, but only negatively that it has not been shown that there is a difference. This I shall do by constructing, in the indicative, cases to parallel his imperative examples. I shall deal only with 'not', 'and' and 'or'.

'Not' has already been partly covered. I have shown that indicative sentences too can be 'cancelled' as well as negated. It only remains to point out that *provided that we confine ourselves to negation* (which is the proper function of plain 'not' in both moods, 'cancellation' being a more indirect proceeding which requires much other logical apparatus besides) singular imperatives are as 'two-valued' as singular indicatives. This is seen by comparing the following two questions, both of which are perfectly legitimate:

Tell me, am I to wear my rubbers? Yes or No?

Tell me, am I going to wear my rubbers? Yes or No?

They would not be legitimate in either case, if a third answer were possible besides 'Yes' and 'No'. But it is not. In both cases, if we answer 'Neither' (which a 'three-valued' logic would allow) we say something incomprehensible—for we cannot in the first case tell Donald neither to wear his rubbers nor not to wear them, any more than in the second case we can say that he is going neither to wear them nor not to wear them. The only thing that 'Neither' might conceivably mean is 'I'm not giving either answer': but this is not to answer the question, but to refuse to answer it; and this can be done to both questions.

In the case of 'and', Professor Hall says that from 'The prisoner was first handcuffed and then given a private audience with the judge', we can infer 'The prisoner was given a private audience with the judge'. But we do not, he says, want to have 'and' used in imperative logic in such a way that from 'Let the prisoner first be handcuffed! and let the prisoner then be given a private audience with the judge!' there follows 'Let the prisoner be given a private audience with the judge!'. The source of his disinclination to allow this inference is perhaps as follows: he envisages a policeman, on being given the first order, making the inference and then carrying out the second order by giving the prisoner an audience without handcuffs. In the case of 'or' a very similar example is given. Professor Hall says that 'The defendant was sentenced to death or was set free' follows from 'The defendant was sentenced to death'; but that we cannot allow an inference from 'Hang the defendant!' to 'Hang the defendant or set him free!'.

Has he forgotten that, in all inferences where the entailment is not mutual, the premisses say more than (are more definite than, exclude more possibilities than) the conclusion? Thus in the

inference from 'Socrates is a male animal' to 'Socrates is an animal', something which is said in the premiss is left unsaid in the conclusion, namely, that Socrates is male; but this does not make the inference invalid. What is left out may in certain cases be something of the last importance. Professor Hall has given examples of such omissions in imperative inferences; but they can occur in indicative inferences too. I ask a policeman in court 'What happened while I was having lunch?' He replies 'The prisoner was first handcuffed and then given a private audience with the judge'. It happens that the judge's wife, who is hard of hearing, has come in with me; she asks 'What happened?'. I reply 'The prisoner was given a private audience with the judge'. The inference is valid, though the conclusion might alarm. In the imperative example, likewise, the policeman who, having been ordered 'First handcuff the prisoner and then give him an audience!', passes on to a subordinate the order 'Give the prisoner an audience!' is guilty, not of an invalid inference, but of omitting an important part of his instructions. And similarly in the disjunctive case: I ask a policeman outside the gaol near which I live 'What has been done with the defendant?' [whom we may assume to be a brutal murderer]. He tells me 'The defendant was sentenced to death'. But although the inference that therefore the defendant was either sentenced to death or was set free is perfectly valid, I do not go home and bolt all my doors and windows because of the possibility that the defendant is at large. This possibility was excluded by the premiss which I know to be true; the fact that it is not excluded by the conclusion which I have rightly inferred does not make the inference invalid. So also in the imperative. If a hangman is told 'Hang the defendant!' there is nothing in logic to prevent him inferring that he is to hang the defendant or set him free. But if he then sets him free, he is guilty, not of a logical error, but of acting on the less definite order when what he was given was the more definite.

Thus Professor Hall is unsuccessful in establishing a difference between the logics of singular imperatives and singular indicatives. He has perhaps been misled by his assimilation of ordinary imperatives to 'ought'-sentences. These latter have, indeed, a logic which is radically different from that of singular sentences, indicative or imperative; it is much more like that of universal or of modal sentences. And this makes his reduction of 'Give me that book!' to 'You ought to give me that book' quite impossible. But I will not repeat here what I have written elsewhere about the differences between singular imperatives and 'ought'-sentences.<sup>1</sup>

Professor Hall's analogy between 'legitimate' and 'true' is of value up to a point, but dangerous thereafter. I do not know whether he himself oversteps the limit of discretion. The danger is that the analogy may lead us to think of legitimacy as a kind of

<sup>1</sup>*Language of Morals*, pp. 191-192, *Ar. Soc. Supp.* Vol. XXV, pp. 201 ff.

erents truth: the 'positivists' have reft from us the comforting conviction that value-judgments may be true; but can we not evade moral disaster by holding that they may be legitimate? And can we not establish that they are legitimate with as much assurance and by methods as certain as those by which we establish that factual statements are true? But the evasion fails in its purpose. For let '---' be a value-judgment and let '...' be a statement of fact. Then "'...' is true' will also be a statement of fact. But if 'legitimate' is the analogue of 'true', it follows similarly that "'---' is legitimate' is, like '---' itself, a value-judgment. But if so, then, if value-judgments are held by anyone to be slippery customers, he will also hold that "'---' is legitimate' is a slippery customer. And therefore 'legitimacy' is no short-cut to 'objectivity'.

The last chapter of the book is a most interesting discussion in retrospect of the problems of method which the rest of the book has encountered. In it Professor Hall gets nearer than at any other point to an understanding of what he is about. Indeed, we may conjecture that the book was written, in the order in which it is printed, during a period in which the author's thought was rapidly developing. This is the only explanation that can be given of how an author who in his last chapter can write with penetration and even lucidity about the relations of language to the 'non-linguistic world' and of ideal languages to ordinary language, can in his earlier chapters make such heavy weather of these same problems. The book, we may feel, records Professor Hall's progress as he has gallantly fought his way out of the jungle; many readers will regret that he has brought out so much of the jungle with him.

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## X.—NEW BOOKS

*The Interpretation of Plato's Republic.* By N. R. MURPHY. Oxford University Press. Pp. 246. 18s.

MR. MURPHY's intention was to re-examine the text of the Republic in order to decide "what Plato said" as contrasted with what he is sometimes supposed to have said"; and one of the great merits of this book is that Mr. Murphy really does manage to have a fresh look at Plato's argument. He takes due account of what other scholars have said, but goes his own way without too much anxiety about his own position *vis-à-vis* others who have worked the same field.

On the whole Mr. Murphy concerns himself with the interpretation of the Republic, and not with the truth or falsity of what Plato says; but he does declare war on Prichard's "brilliant but hostile" studies of Plato's ethical theory, and much of the first half of the book is relevant to this controversy. In an article in *MIND* for 1912, and in 'Duty and Interest', Prichard attacked Plato for accepting the challenge of the Sophists and trying to show that a man will not in the long run lose by doing what is just. Of course Plato did accept this challenge, and set out to refute what Thrasymachus had said *τὸν τοῦ ἀδίκου βίον φάσκων εἶναι κρείττω ἢ τὸν τοῦ δίκαιου* (*Rep.* 347 e). So that when Prichard talks about showing that just action is to the agent's advantage, we can find apparent equivalents in Plato's words. The question is whether Plato is really doing what Prichard's language would suggest. We do not know exactly what Prichard would take to be showing that something was to a man's interest or advantage, but must suppose it to be connected with getting him what he wants, or else what would make him happy. According to Mr. Murphy we will misunderstand Plato if we read him in this way. "Plato holds . . . that acts are right if they promote the agent's real interest. He is speaking of a total interest and of what really is our interest and really is good, whether we think so or not either at the time of acting or subsequently. A man's real interest is thus distinguished from and often contrasted with both what would satisfy and what he thinks would satisfy the desires he actually feels." (p. 92) What we want to know is how we can understand such a concept of "real interest". Mr. Murphy seems to be right in saying that we cannot suppose that whenever Plato says that something is to a man's advantage he means that it gets him what he wants in the long run. (Several passages in the *Gorgias* confirm this statement, e.g. 512 b, where Socrates says that it is better for a man to die than to live wickedly—*οὐκ ἀμεινὸν ἐστὶν ζῆν τῷ μοχθηρῷ ἀνθρώπῳ· κακῶς γὰρ ἀνάγκη ἐστὶν ζῆν*—and the argument of *Gorgias* 470-481.) As Mr. Murphy says "Plato might fairly enough be represented as getting his conclusions by starting from some such conception of interest and then transforming it through the introduction of a fresh principle by which a man's 'interest' becomes his 'good' . . ." (p. 93). One might say that Plato was trying to draw his hearers towards a moral notion of *τὸ συμφέρον*, as well as to meet them on their own ground, and if this is true Prichard's attack fails, for we do not know what is to a man's advantage until we know how he *should* live.

Perhaps Mr. Murphy would not agree with this interpretation of what he calls 'real interest' or a man's 'good', though it would fit well with

what he says in an excellent note on the meaning of *εὐδαιμονία*. In dealing with Book IV he returns to a more usual interpretation of 'interest', and develops some of the least satisfactory strands in Plato's doctrine. 'Reason' becomes not merely a separate part of the soul, but one with its own satisfactions. "To satisfy reason we must conform with its standards, and our own feelings of pleasure and pain do not provide a criterion of success" (p. 59). This is extremely confusing. Using the ordinary sense of 'satisfied' Mr. Murphy admits that a man might be satisfied even when living a life which was not that of Plato's just man. But then he starts to speak of a part of a man's soul as if it could make demands, have satisfactions, etc., which *he* does not, but which are ascribed to him because the soul is his. ("Plato would not allow that the soul, or the man himself, could be satisfied until the principles implied by this function of reason were obeyed" p. 58.)

Mr. Murphy also tries to answer critics such as Prichard by suggesting that an ethical system in which "duty and interest are heterogeneous and unrelated" cannot allow for the fact that most people feel justified in pursuing their own interest or part of the time. The argument is most obscure, but seems to depend on the assumption that his opponents think of duty as making unlimited claims upon a man's time, in which case one would have thought that it was their *code* which did not fit with that of the average man. But Mr. Murphy keeps this notion of duty (mistakenly connecting it with the denial that the "incidence of morality" is intermittent) and then argues that there must be a co-ordinating authority to keep some place for interest. The curious thing is that the argument is in a way close knit, but the materials are so unsatisfactory (e.g. 'duty' sometimes as a 'motive', sometimes an action, 'interest' sometimes the pursuit of happiness, something anything done without thought of duty) that no good can come of it.

In some parts of the book Mr. Murphy has some most helpful paragraphs on the correspondence of particular terms in English and in Plato's Greek, but where he is dealing with the argument with Thrasymachus, and the request made by Glaucon and Adeimantus, we need a more radical analysis to resolve the difficulties. He will say, for instance, that here a term is, and here is not, being used in an ethical sense, but as we gain no clear impression of what he means by this, we are given no help where we need it most, that is, in deciding what kind of a dispute is going on in different parts of Books I-IV. Moreover Mr. Murphy seems positively misleading here. He says that once "the special fallacy" of Thrasymachus ("that justice is not a duty") has been disposed of *δικαιοσύνη* regains its ethical sense; but this is doubtful. Prichard seems nearer to the truth in recognising that calling an action "just" in the context of sentiments such as those which worried Glaucon and Adeimantus is not properly described as "calling it a just action", though he does not give a satisfactory account of what the threat amounts to, and how it should be met.

Turning to Books V, VI and VII of the Republic, Mr. Murphy has interesting things to say about the distinction between *δέξα* and *γνώσις* and Plato's use of *δοκεῖν* throughout the dialogue. One of his main theses in the second half of the book is that Plato never meant to question the reality of physical things, and this is supported by a skilful use of material from other dialogues. Mr. Murphy makes the helpful suggestion that at times Plato is drawing on the tense significance of the verb *ἵσται* and because of this "will deny *ἵσται* of a physical thing and affirm *γίγνεται*

without in the least intending to question its reality, in the ordinary usage of the English word, but only to draw a contrast in the mode of being real" (p. 144). He also makes detailed suggestions for dealing with the difficulties over the things put between being and not being in Book V. These he thinks should be treated as special cases, and he discusses them under the heading of "relational facts". Mr. Murphy brings justice under this heading by pointing out that when saying which acts are just we may give a class of actions, as Polemarchus did in Book I, and yet we do not think that under *any* circumstances an action which was of this kind (e.g. returning that which has been entrusted) would be just. The right circumstances are taken as the second term in the relation, and the answer to "are such acts just?" is said to be "yes and no". Obviously this is different from the case of something which is large in relation to some things, small in relation to others, and the term "relational characteristic" hardly seems to have a clear sense when used to cover largeness and justice, but there is an interesting point here. When we say that a certain kind of action is right or wrong, do we mean to say that every action answering to the explicit specification would be so? If not, an ethical 'generalisation' is not refuted by producing any and every apparent counter example. The question of what is a counter example is not settled until we know whether there is an implicit reference to standard conditions, and if so what. Mr. Murphy does not deal with the problem of how ethical principles relate to particular actions; he speaks of "quasi-specific types" and leaves it at that, and this makes it difficult to deal with later sections in which he is trying to construct existential propositions about "just types".

Mr. Murphy does not try to give the same analysis for other passages in which apprehension of the many is said to be deficient. "It need not be assumed", he says "that Plato has but one single reason for holding that τὰ πολλά are not intelligible. He seems in fact to have a variety of reasons applicable to different cases" (p. 124). But he does think that all that Plato seems to say about the unreality or imperfect reality of the many can be explained on a single principle which "commits him neither to the statement that reality permits of degree nor that physical things and events, whether they do or do not participate in this or that form, are anything less than real. The general rule on which he is working seems clear enough. Whenever we speak of an unreality it is not with reference to things but to false thoughts" (p. 129). Perhaps there is no objection to saying that Plato is dealing with false thoughts, if like Mr. Murphy we give a very generous interpretation to "false", making it cover all sorts of deficiencies, the need for qualification, etc. But when Mr. Murphy tries to get back to the question of reality by producing existential propositions about "instantiations" his account is logically so unsound that we would be better left with the obvious obscurities of Plato's text.

When he comes to the Sun, the Line, and the Cave, Mr. Murphy is once more in his element. In a general way he follows critics such as Professor Ferguson in dealing with the lower part of the line—treating the distinction between *eikasia* and *phantasia* as "illustrative" and not trying to locate the actual states corresponding to these divisions. But he introduces variations at several points. In the first place he denies that the Line contains a simile of a divided line at all, saying that what we are to divide is the objective material itself, and backing this up by a detailed examination of the text. To explain the introduction of the words "as

a line that has been bisected" Mr. Murphy has recourse to the explanation that Plato wants to unify the four classes before dividing them, which seems strained. Since he wants to minimize the importance of the line, Mr. Murphy has to deny that the relative lengths are important, and he suggests that the words *ἀνισα τμήματα* should probably be deleted as a gloss which has crept into the text. The difficulty is to see what can then be made of 534a (the statement of ratios) for it seems to be only the unequal bisection of the line which gives us the relation which can hold between *νόησις* and *δόξα*, *ἐπιστήμη* and *πίστις*, and *διάνοια* and *εἰκασία*. Mr. Murphy says that "Plato's statement here does not seem to mean more than that *ἐπιστήμη* bears the same relation with *πίστις* as *διάνοια* with *εἰκασία*; i.e. both the first pair are superior in their kind and the second inferior" (p. 159). To support this interpretation Plato would have had to have said that as *νόησις* is to *δόξα* so is *ἐπιστήμη* to *διάνοια* and *πίστις* to *εἰκασία*, but what he did say was *ὅτι νόησις πρὸς δόξαν, ἐπιστήμην πρὸς πίστιν καὶ διάνοιαν πρὸς εἰκασίαν*.

The rest of what Mr. Murphy has to say about the Sun, the Line and the Cave does not stand or fall with this piece of interpretation; he might in any case be right in putting forward a "tripartite" version of the Line, holding that "Plato is seriously concerned in the end with three levels and only three, *δόξα*, *διάνοια*, *νόησις*; or alternatively with *ἀπαιδευσία* and the two levels of *παιδεία*, mathematics and philosophy". But there are difficulties in his way of synthesising the Cave and the Line, for he wants to identify both the stage of looking at *ἀνάλματα* in the cave, and the stage of seeing shadows in the outside world with *διάνοια*. This is in line with 532b where Plato is speaking of the role of mathematics in education, for there the study of mathematics is identified with the *whole* ascent from the shadows in the cave to the real objects in the sunlight. But an interpretation which brings the division between *δόξα* and *γνώσις* within the cave will not do for the earlier section in which Plato certainly seems to identify the cave with the dim world of becoming, on which the light of truth does not shine. Mr. Murphy has to deny that there is any change of level when the prisoner leaves the cave; he takes the change from the light of the fire to the light of the sun to be an advance, but supposes that this is offset by a descent from original to image as the *ἀνάλματα* are exchanged for the *φαντάσματα* in the outer world. This point must depend on a rather literal interpretation of the passage on the division of the lower line, taking the relation introduced to be that between shadow and solid rather than between copy and original, for otherwise there would be no reason to say that the change from *ἀνάλματα* to shadows and reflexions (from copy to copy) represented a descent, as from *πίστις* to *εἰκασία*. Very good reasons would have to be given for rejecting the suggestion that it is as he leaves the cave that the prisoner leaves the world of becoming, and the only really cogent one which Mr. Murphy has given is the argument from 532b, where Plato is talking about education by means of mathematics. It is in fact impossible to do justice both to 517b and to 532b by a single interpretation. In the earlier passage, which looks back to the Sun metaphor, and to the discussion of *τὰ πᾶλλα* in Book V the important division is that between *δόξα* and *γνώσις*, and this is represented dramatically by the ascent from the cave to the daylight. (Mathematics must then be represented by the images outside the cave.) Later the important thing is the part which mathematics can play in education, and there is every reason for representing the beginning of mathematical study by the turning round

## NEW BOOKS

of the prisoner from the wall of the cave. (Everything between the original shadows and the real objects in the sunlight must then be identified with the objects of *δαίμονα*.) Would it not be better to read the Cave allegory now in one way, now in the other than to insist on a single scheme? Do we really think that there is something ('Plato's thought') which will emerge when everything is fitted together, even if we have had to scrap some striking image which gives a passage its literary force?

It would be hard to read this book without being grateful for the number of good things that are in it, and in particular for the way Mr. Murphy uses material from other dialogues while keeping a sense of context. It is only when he interprets Plato in terms which are themselves technical and obscure that Mr. Murphy ceases to be helpful.

PHILLIPA FOOT.

*Symbolic Logic, An Introduction.* By FREDERIC BRENTON FITCH, Professor of Philosophy, Yale University. The Ronald Press Co., New York. \$4.50.

THIS book is intended 'both as a textbook in symbolic logic . . . and as a treatise on the foundations of logic'. I do not think it could effectively serve the first purpose; the choice of subjects, and the manner of presentation, are too individual to give a reader a balanced account of this field of research. Moreover, from a pedagogic point of view the treatment is excessively abstract; the author hardly ever illustrates his long trains of formal proofs by concrete examples. The book must rather be regarded as a study of logical fundamentals, addressed to specialists.

Undoubtedly the best parts of the book are the sections devoted to propositional calculus and quantification theory. The law of excluded middle is not assumed; this has the advantage of enabling the author to bring out the precise relations between his own system and ordinary two-valued logic and Heyting's formalization of intuitionist logic. Like Quine's *Methods of Logic*, the book develops techniques of 'natural deduction' based on the work of Jaśkowski and Gentzen; this brings clarity and simplicity into quite complicated proofs.

The logic of classes and relations in this book hardly goes beyond the so-called calculi of classes and relations, which, as is well known, add nothing essentially new over and above quantification theory. Further developments, e.g. a treatment of the ancestral relation, are reserved for a promised volume on foundations of mathematics. The logic of definite descriptions is not discussed at all.

Modal logic is developed in a rudimentary way; hardly any use is made of it or indicated for it. We are told that all true identity statements are necessarily true (§ 23.6). The difficulties that arise over this matter, which have been discussed e.g. by Quine and Carnap, are not mentioned.

The author frequently claims that his system is *philosophically* superior to *Principia Mathematica*; it is clearly not his intention to present it as a piece of technical mathematics on which philosophers cannot properly comment. I shall therefore list, with a minimum of comment, some leading views of the author's, so that readers may judge for themselves whether this claim can be upheld.

(i) It is throughout assumed that every sign has some definite *thing* as

its meaning. (This goes for the symbols replacing "and" and "or" and "not"; but not for a right-hand or left-hand bracket.) No distinction is made between complete and incomplete symbols, or between sense and reference of expressions. A sentence is a string of names, and is itself a name.

(ii) Like Frege, the author wishes to give a sense to hypotheticals even when the antecedent and consequent are not sentences. He differs from Frege, however, in not stipulating what the sense shall then be; he thinks he has supplied a sense merely by requiring that there shall be one (§ 21.9).

(iii) Since every sign means a thing, the letter "x" used in the symbolic context that corresponds to the phrase "the class of all *xs* such that . . ." must be interpreted as a name—e.g. as meaning the same as the name "Napoleon" or "the North Pole" (these are the author's own favourite examples). But on the other hand, we are told that although "x" may here be taken to mean the same as "Napoleon" and may accordingly be replaced by "Napoleon", such use of the name "Napoleon" is not mention of the man Napoleon (§ 17.13).

(iv) The use made of dropping the *tertium non datur* is that now the proposition that Russell's class is a member of itself can be treated as not being either true or false. But a non-classical theory of negation will not resolve Curry's paradox. Curry shows that, on the naive view of classes (viz that *x* belongs to the class of all *Fs* if and only if *x* is an *F*), any arbitrary proposition *p* can be derived by *modus ponens*; negation does not enter into the proof. Our author's solution is that *modus ponens* is not unconditionally valid; the validity of a proof depends on how the premises were got (§§ 18.1, 18.4, 18.5). It is described as a "fallacy" to assume unquestioningly that two proofs severally valid will form a valid proof when "placed end to end" (§ 20.42).

A consistency proof is offered for the system; this proof is given in words, not symbols—the author relies on our *seeing* what follows from what. But if *modus ponens* is not unconditionally valid; and if two proofs severally valid may form an invalid proof when combined; how can we be sure that an informalized consistency proof is flawless? This difficulty might have been expected to appeal to the author; for his Appendix C is largely concerned with "self-referential inconsistency"—i.e. the unwisdom of sawing through the limb you are out on.

(v) When philosophy is being attempted, even technical mistakes in mathematical logic get by. The author says that Cantor's diagonal proof is self-referential, in that it defines a real number in terms of the class of all real numbers. This is quite wrong; what the proof shows is that for any given *denumerably infinite* class of real numbers there is a real number not belonging to that class; and if the enumeration of the given class is *effective*, this new number can be written down *decimal place by decimal place*, so that there is no doubt of its existence. There is no question here of bringing in the class of all real numbers. (This is like Euclid's proof that for any prime number *n* there is a prime greater than *n*; the proof shows us how to reach a new prime given the primes up to *n*—not, given "all" prime numbers.)

The index of names does not include either Frege or Wittgenstein.

P. T. GRACH.

*Introduction à l'Épistémologie Génétique.* By JEAN PIAGET. Paris: Presses Universitaires de France, 1950. Tome I, La Pensée Mathématique. Pp. 361. 700 fr. Tome II, La Pensée Physique. Pp. 355. 700 fr.

THE work of Jean Piaget is well known in this country through his experimental studies on the thought processes of the child. What is not so well known, however, is that during the last decade or so, he has carried out a large amount of work on the problem of the development of mathematical and physical thought in the child, and has constructed an axiomatic system which he claims is applicable to the structure of intellectual activities (cf. *Traité de Logique* 1949). Piaget's work deals with the development of such concepts as classes, relations, numbers, space, time and causality—in short with the Kantian categories. His *Introduction à l'Épistémologie Génétique* contains an account of these studies together with a summary of his formal system.

It is not possible within the limits of this review to enter upon a detailed discussion of such a wealth of interesting material, experimental as well as formal; the most we can do is to introduce the English reader to some of its more important features. Throughout his work he places considerable emphasis on the psychological aspect of logical and mathematical relations, on the way we actually handle symbols and formulae. From this point of view, Piaget's account bears a marked resemblance to the views of Poincaré and the intuitionists; the construction of number has for Piaget a definite psychological aspect, which he claims has been dismissed by Russell and his school as irrelevant. He does not, however, identify logico-mathematical systems with the psychology of thought processes; a sharp distinction is drawn between them, they are related together as an abstract formal system is to its interpretation by some particular science.

Piaget, who like Aristotle, started life as a zoologist, compares his approach to that of the embryologist; he believes that a study of child development may throw light on adult thought structures just as a study of embryology has brought to light similarities of structure in the animal kingdom. For this reason, he finds little help in the historical-critical method used by historians of science, since they largely concern themselves with adult types of thinking. The aptness of Piaget's analogy may, of course, be challenged, since genetic questions are nowadays thought to be irrelevant to logic and epistemology. To this, he would no doubt reply, that since intellectual activities can be shown to be thus formalisable, a careful study of these logical systems at their simplest level (in the child) will enable us to obtain a better understanding of the structure of adult thought. However, Piaget's argument loses a good deal of its force if one refuses to accept the isomorphism between thought and logic which he posits.

Most of Piaget's information about child thought is obtained through a skilful clinical method of questioning backed up by simple experimental procedures. The basis of his view, and here it is in keeping with modern pragmatic tendencies, is that logical and mathematical notions first show themselves in the child as overt activities, only at a later stage are they internalised and take on a conceptual character. They are to be conceived as scaled down internalised actions, in which things are replaced by signs and actions by operations on these signs. Rational activity occurs

when the child's trial and error gropings attain what Piaget calls an *equilibrium*—a definite pattern or order, which may be inverted in thought. If the child now makes a mistake when performing a task he is, in this way, able to return to his starting point by simply inverting the order of his activities. Though Piaget certainly recognises a difference between particular overt activities and general conceptual patterns, is this difference merely one of internalisation? The concept of internalisation seems to cover a multitude of sins. Do the signs of growing *reversibility* in the child's behaviour, indicate anything more than an increase in his capacity to grasp universals?

Piaget claims to have discovered a parallelism between some of the operations which constitute a mathematical group, and the system of psychological operations (*groupements*) by which the child compares, distinguishes and orders the objects around him. What Piaget is doing, however, is finding analogies, not as the older thinkers did between the laws of traditional formal logic and actual thought processes, but between them and the postulates of group theory which, of course, include these laws. His comparison then amounts to much the same thing as that made by the older idealist logicians such as Bosanquet, except that it is couched in terms of a behavioural isomorphism. Piaget's contention is that the elementary logical and mathematical behaviour of the child, consists in the creation of complex systems of dichotomous divisions, and that logic and mathematics develop out of this. What is of interest here is that the behaviour of the child comes to resemble pretty closely the activities nowadays associated with digital computers. The arithmetic of the child is essentially additive in kind, multiplication and division is reduced to repeated additions and subtractions. Mathematicians and others interested in mechanical minds will therefore find in Piaget's work some confirmation of their views.

Piaget is critical of present day symbolic logic, with what he calls its discontinuous and atomic mode of description, and the attempt of Russell and others to dissociate logical research from psychological analysis. In the light of the Gestalt findings he thinks we must construct a logic of wholes (or *groupements*) if we want to formalise the conceptual activity of the mind. He claims that the abstract system he has constructed constitutes such a logic. But Piaget's argument is, of course, no argument against logic using atomic elements, if it is simpler for it to do so. The need for a logic of wholes stems from his assumption that there is an isomorphism between logic and the psychology of thought and that logic is closely related to, if not subsumed under, the mathematical theory of groups. Piaget's logic of wholes, on examination, turns out to refer to complex systems of classes and relations which are identical with the atomic elements postulated in *Principia Mathematica*. The only difference seems to be, that unlike the logistic school, Piaget does not assume a simple one-way relationship between arithmetic and logic. They are both, he believes, related to group theory. Most of us, however, would agree with the practical implications of Piaget's view, that in order to analyse conceptual activities we require complex relational structures rather than simple dyadic relations.

The reader may perhaps have been puzzled by Piaget's use of such concepts as *equilibrium* and *reversibility*, with their overtones of thermodynamics. Perception is, however, conceived by him as a statistical process, since it consists in the progressive correction of the crude data given in perception. Actual vision constitutes a sort of sampling as only

certain parts of the perceived figure are fixated whilst others are neglected. Take for instance the simple case of two lines where there is a considerable difference between them, the larger of the two will catch the eye more frequently and hence appear larger. For the Gestalt psychologist however, the perceptual constancies such as shape and size, are not the product of sampling procedures, they belong directly to the perceived object and are independent of age and ability. Piaget is of a different opinion, his experiments have shown that the perceptual constancies are only built up gradually. The young child does not attribute a constant size, or even identity to the objects around him. Indeed, their absence among backward children, has been used by one of his students as a test for sorting out different types of mentally defective children.

In his second volume, Piaget examines in some detail the development of the physical concepts of time, speed, atomism, conservation and causality, all of which are taken as constructions from behavioural activities. Starting as he does, from the facts of observable child behaviour rather than adult introspections, he differs from thinkers like Mach, in his emphasis on the part played by overt activities in building up the conceptual machinery of thought. In his discussion of causation Piaget thinks he has shown that causality first arises from sequences of events in which the activity of the child is always an element—elsewhere he has referred to this as the ego-centricity of child thought. In order that an event *A* should be considered as the cause of an event *B*, it is necessary that *A* be an action of the child itself; he has to push, pull, or otherwise handle the object. Only at a later stage does causality become dissociated from the child's activities and take on a physical character. Piaget's view is to be distinguished from the so-called 'activity theory of causation' since the sensations of effort, which arise when we push against objects are, for him, merely the signs of such movements and resemble them as much, or as little, as words do the objects they denote.

How is Piaget's work to be evaluated? Though it is no doubt dangerous to read adult activities and ways of thinking into the child, we do not have to discount them altogether. One of the difficulties of the genetic approach, which makes it less appropriate to human beings than to animals, is that many of the really important intellectual activities, notwithstanding the intelligence tester, do not appear until maturity. No doubt it is misleading to judge the behaviour of the child by that of the adult, but it is just as misleading to judge the behaviour of the grown up by that of the child. Without falling into an outdated Kantianism, can we not assume that some intellectual functions are inherent in the individual? The child's acts of dividing, combining and ordering, which Piaget claims give rise to logical concepts, seem to be directed by some glimmer of insight into simple logical relations or universals. The child does not just act in an unconscious manner, he also notes that some things resemble each other, that others are dissimilar, or have a certain order about them. The question of the priority of conceptual thought and practical activity, is perhaps an unanswerable one.

On the other hand, even in his most logical moments, man is not as rational as Piaget sometimes makes him out to be; that there is an isomorphism between the child's overt behaviour patterns and certain types of logical system is undoubtedly of interest, but it does not follow that the psychological antecedents have also such a clear cut logical character. This is simply to confuse thought with its behavioural products. What Piaget seems to be trying to show is that logic and mathematics are built

up by the child from simple overt activities analogous to that used by modern digital computers. Many mathematicians would reject such a view, pointing out that the simple operations performed by children and digital computers, are not really mathematics as we know it in its higher branches. They would claim that at this level value judgments of a complex kind enter into its construction.

There is no question that Piaget has carried out a valuable enquiry into the field common to psychology, philosophy and logic. No other contemporary psychologist has examined the foundations of his subject with such thoroughness, skill and erudition. His work should be of help to philosophers and others, who wish to know something about what modern psychologists are doing in the field of concept formation, as well as acquainting psychologists with the work of philosophers and mathematicians engaged in similar studies. In symbolic logic psychologists have at their disposal a tool in some ways as important as statistical method. Piaget has clearly demonstrated its possibilities.

W. MAYS.

*Peirce and Pragmatism.* By W. B. GALLIE. Penguin Books. 1952. Pp. 247. 2s. 6d.

THIS addition to the admirable Pelican Philosophy Series removes any excuse people may have had in the past for being unacquainted with the thought of C. S. Peirce. For there is now available at a price within reach of everybody a concise and readable introduction to Peirce's leading ideas. Professor Gallie has chosen to present these ideas from the standpoint of the pragmatic maxim in its original role as a method of logic. His choice was dictated by the belief that Peirce is "the one unquestionably great figure in the Pragmatic movement", and that "his general philosophy, of which his Pragmatism is an essential facet, seems likely to prove of more lasting value than the philosophical movement to which he gave an initial impetus and a name" (p. 13).

It is a cause of cheerfulness that at long last a British discussion of pragmatism has appeared which does not limit itself to the views of William James. James's pragmatic theory of truth used to be a standard target for critics such as Bradley and Moore, and it was philosophical child's-play for them to shoot holes in statements like "the true is the expedient", "truth is what works", etc. Peirce's pragmatism, which they ignored, is not so easily disposed of. His subtle and tough-minded doctrine differs *toto caelo* from the oversimplified doctrine of James, as Professor Gallie makes clear in his opening chapter. Peirce sought to produce not a theory of truth but a logical device for ensuring that the abstract concepts which philosophers love to use should have some concrete, experiential meaning. All concepts which could not be given such meaning were to be dropped as empty verbiage, and the fruitless controversies based on them thereby brought to an end. Once this was done philosophy would be able to enter the sure path of science.

Unfortunately, Peirce did not stick to one formulation of the pragmatic maxim, and so it appears in a variety of forms throughout his writings. Professor Gallie discusses two of these forms, which he calls the "wider" and the "narrower" versions of the maxim. According to the former, pragmatism is simply a rule governing the choice of hypotheses. It states "that every genuine hypothesis must have verifiable

consequences. If we genuinely understand a given hypothesis, we must know how *some* evidence could be produced for or against it" (p. 185). This version represents for Gallie "a most useful, because flexible, tool of criticism, which can be directed with equal effect at the key terms, the pronouncements and the arguments (or seeming arguments), of any piece of discourse" (p. 142). According to the narrower form of the maxim, the entire meaning of any conception or hypothesis is made up of *all* the necessary practical consequences which follow from it. Gallie contends that this view imposes an excessively severe restriction even on the discourse of well-advanced sciences, and "cannot possibly be applied to hypotheses which we employ in history and in the course of practical life" (p. 177). He also contends that Peirce did not clearly recognize the difference between the two versions of pragmatism, though the narrower version predominates in most of his papers. Indeed, towards the end of his life he tended "to push his narrower interpretation to the limit—to the point at which the impossibility of regarding it as a *general* principle of logic becomes all but obvious" (p. 171).

While the above ambiguity is certainly worth discussing, I doubt whether it is as important for the understanding of Peirce as another ambiguity which Professor Gallie does not mention. This arises from the use of the expression "practical consequences" in the statement of the pragmatic maxim. Sometimes Peirce seems to say that the practical consequences of a conception are simply the set of operations to which it gives rise together with the perceptible effects which follow from the performance of the operations. He reports that he learned this doctrine in the laboratory (see his example of the definition of *lithium*, quoted by Gallie on p. 16), and he recommends its employment in philosophy. At other times, however, he appears to mean by "practical consequences" only the factors which promote "the development of concrete reasonableness; so that the meaning of a concept does not lie in any individual reactions at all, but in the manner in which those reactions contribute to that development". Here the pragmatic maxim is made to serve Peirce's speculative interests, and is thought to justify in some obscure way a metaphysics of cosmic evolution.

Had he explored this more important ambiguity, Professor Gallie would, I think, have been able to throw more light on the relation between Peirce's theory of knowledge and his metaphysics. These are the other two subjects dealt with in the book. The former of them is summarized in three excellent chapters which take up the assault on Cartesianism, critical commonsensism, and the doctrine of thought-signs. The only thing one misses here is some indication of Peirce's debt to Kant, which is particularly evident in the papers of 1868. On the whole, the statements occurring in Peirce's theory of knowledge conform to the requirements laid down in the "wider" version of the pragmatic maxim. But these requirements are not met, according to Gallie, by his metaphysical statements. "We cannot admit the suggestion that any of Peirce's metaphysical statements function as genuine hypotheses . . . they are not in any way informative; they tell us nothing" (pp. 179-180). Yet they are not on that account valueless. Gallie suggests that they perform the useful function of adumbrating or directing our attention towards "certain new styles of description and explanation of fact, which the actual conditions of our empirical knowledge can be shown already to require" (p. 180). This is an interesting suggestion; but it receives little elaboration in the two chapters which outline Peirce's metaphysical

categories and his speculative cosmology. Both of these doctrines are judged to be philosophical failures, at variance with the teachings of pragmatism. The reader is left in the dark, however, as to the reasons for the discrepancy between the epistemological and the metaphysical parts of Peirce's thought.

In view of all this, it is odd to find Professor Gallie saying that "the greatness of Peirce . . . lies . . . in the organic unity of his thinking" (p. 32), and urging that "what is particularly needed . . . is a demonstration of that organic unity . . . which the accidents of publication have rendered obscure" (p. 44). My guess is that readers of the book will not get from it an impression of Peirce which agrees with this judgment. Moreover, many students who have burned much midnight oil over the *Collected Papers* have been unable to locate any such "unity". What readers of the book will get, I think, is an impression of Peirce as a mind of great analytical power and originality, attacking problems along a very wide front, and reaching results which are generally well worth pondering. Professor Gallie has thus made a welcome contribution to the campaign aimed at rescuing Peirce from the undeserved neglect accorded him in his own lifetime.

T. A. GOUDGE.

*Social Theory and Social Structure.* By R. K. MERTON. The Free Press of Glencoe, 1951. Pp. 423. 40s.

PHILOSOPHERS tend to regard sociology as a bogus subject; indeed it was recently described as a contemporary "malaise" at a philosophical conference. This impression is often confirmed by dipping into American works which specialize in pretentious, scientific terminology and premature systematization. This book, however, seems to be different. It is a collection of articles dealing with methodological and empirical problems in sociology, many of which are of philosophical interest.

The book starts with a methodological section, the most interesting chapter of which consists in an examination of the different senses of the term "function" employed by anthropologists and sociologists who give functional explanations of institutions and social practices. This discussion of current usages is a preliminary to outlining the author's own suggestions for the proper procedure of functional analysis modelled on Cannon's method of physiological analysis. This leads on to the distinction between latent and manifest functions, purposes, motives, unintended consequences of actions, and other terms used widely in social science. The manifold problems of teleological explanation are presented in a real, if complex, context, and are illustrated by concrete examples taken from the work of scientists like Malinowski and Ratcliff-Brown.

Part II contains some studies in social structure which are stimulating and suggestive. In a chapter on social anomie cultural goals (e.g. monetary success) are distinguished from prescribed procedures for attaining them, and the consequences for the individual and for society of stress on one or the other or on both or on neither are examined. It is maintained, for instance, that a society becomes unstable when the value of certain goals is stressed without any corresponding stress on prescribed procedures for attaining them. This is surely a novel aspect of the problem of the means-end relation which is illustrated by some interesting material. Two chapters deal with bureaucracy, one of which discusses

the role of the intellectual in bureaucracy while the other considers the effect of bureaucracy on personality. These are followed by a most stimulating chapter called "The Self-Fulfilling Prophecy" which illustrates and discusses what the author regards as one of the basic theorems in social science: "If men define situations as real, they are real in their consequences." Having shown by examples of runs on banks, race tensions, and in-group and out-group virtues and vices, how human affairs differ from the world of nature in that public definitions of a situation become an integral part of the situation, the author suggests that "the self-fulfilling prophecy, whereby fears are translated into reality operates only in the absence of deliberate institutional controls." Philosophers are familiar with the self-fulfilling prophecy thesis in the context of the free-will problem; but it is exciting to see it exalted to the position of a theorem and illustrated by historical and contemporary material.

Part III deals with problems of the sociology of knowledge raised by Marx, Sorokin, Durkheim and Mannheim, and also presents some studies in radio and film propaganda. Part IV, on the sociology of science, examines concrete empirical problems like that of the relationship between puritanism, pietism and the rise of science, and between scientific problems of the seventeenth century and economic influences. These chapters are straightforward, and of considerable interest to the historian of science and philosophy.

This book is surely the sort of thing one looks for in a rudimentary science—a collection of articles which put forward limited hypotheses for discussion and which deal with methodological problems encountered. The author believes the major task of the sociologist today to be "to develop special theories applicable to limited ranges of data" or, as Professor T. H. Marshall put it, to build "sociological stepping stones in the middle distance". This aptly describes the book as a whole and there seems little in such limited studies to warrant epithets like "bogus" and "pretentious" which are so often applied to social science.

R. PETERS.

*Alciphron, ou le Pense-ménu.* By GEORGE BERKELEY. Introduction et Traduction de JEAN PUCELLE. Aubier Editions Montaigne, Paris, 1952. Pp. 343.

BERKELEY's *Alciphron*, his longest and most elaborate work, deserves the careful attention of anyone who is interested in the religious thought of the eighteenth century; and if it is unlikely now to arouse the enthusiasm with which its first publication was greeted, neither does it merit the impatient condemnations which have sometimes been passed upon it. Like all works of apologetic, it gives the impression of making its way to conclusions long fore-ordained and not seriously questioned; but in Berkeley's hands the level of debate is not allowed to fall intolerably low, and its course is also diversified by wit, by satire, and by stylistic charm. For some reason, the dialogue form is apt in this instance to irritate—perhaps because it provides the virtuous debaters with too many contrived opportunities to score off their opponents; however, neither in this nor in other respects is Berkeley's use of the form much inferior to Plato's.

Professor Pucelle's translation seems to me entirely admirable, and he contributes an introduction of real value. He is not only, though he is remarkably, well versed in the writings of Berkeley and his contemporaries; he has also, I think, caught very exactly the temper of

Berkeley's mind. Inevitably, we find here little discussion of his philosophical (or, in Professor Pucelle's terms, his epistemological and metaphysical) doctrines; but it is useful to be reminded of those religious concerns, hardly ever quite absent from Berkeley's mind, which are apt to be wholly absent from modern discussions. It may be that Professor Pucelle goes rather too far in speaking of Berkeley's "religious pragmatism". For though Berkeley is always inclined to see the value of religion in its effects on human conduct, he never suggests that these have bearing on its truth. Indeed, to speak of pragmatism here at all is something of an anachronism, though certainly one less flagrant than another which Professor Pucelle rightly castigates—an attempt to exhibit Berkeley as addicted to "absolute scepticism" and "absolute despair", a kind of existentialist before his time. This attempt, as Professor Pucelle pleasingly observes, is "hautement fantaisiste". His neologism "Pense-menu" for "Minute Philosopher" is also, I think, particularly pleasing.

G. J. WARNOCK.

Received also :—

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## XI.—NOTES

### TRINITY COLLEGE, CAMBRIDGE

#### PERROTT STUDENTSHIP IN PSYCHICAL RESEARCH

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined, for the purpose of the Studentship, as 'the investigation of mental or physical phenomena which seem *prima facie* to suggest (a) the existence of supernormal powers of cognition or action in human beings in their present life, or (b) the persistence of the human mind after bodily death'.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating, in consultation with a Supervisor to be appointed by the Electors, some problem in Psychical Research. The Student shall not, during his tenure of the studentship, engage in any other occupation to such an extent as would in the opinion of the Electors interfere with his course of research. Residence in Cambridge is not required.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake.

Applications from candidates should be sent to *The Secretary, Perrott Studentship Electors, Trinity College, Cambridge*, not later than 30th April, 1954. Intending candidates should write to the Secretary for further details before applying.

The election to the Studentship will take place in the Easter Term of 1954, and, if a candidate be elected, his tenure will begin at Michaelmas following the election.

#### MIND ASSOCIATION

Those who wish to join the Association should communicate with the Hon. Treasurer, Mr. J. D. MABBOTT, St. John's College, Oxford, to whom the yearly subscription of sixteen shillings (payable in advance) should be sent. Cheques should be made payable to the Mind Association. Members may pay a Life Composition of £21 instead of the annual subscription. The annual subscription may be paid by Banker's Order; forms for this purpose can be obtained from the Hon. Treasurer.

In return for their subscriptions members receive *MIND* gratis and post free, and (if of 3 years' standing) are entitled to buy back numbers of both the Old and the New Series at half-price, if still in stock.

The Hon. Secretary of the Association is Professor KARL BRITTON, Dep. of Philosophy, King's College, Newcastle-on-Tyne.

Members resident in U.S.A. may pay the subscription (\$2.80) to the Hon. Assistant-Treasurer, Professor B. Blanshard, Dept. of Phil., Yale University, New Haven, Conn.

Those who join the Association after previously buying *MIND* direct from the publishers or through a bookseller are asked to inform the Treasurer of this (and of the name of the bookseller) when they join.

JOINT SESSION OF THE ARISTOTELIAN SOCIETY AND  
THE MIND ASSOCIATION

at Magdalen College, Oxford, 9th-11th July, 1954

PROGRAMME

- Friday, 9th July, Address by Professor H. H. Price.  
at 8 p.m.
- Saturday, 10th July, (a) Symposium: "Can Effects precede their  
at 10 a.m. Causes?"  
Mr. M. Dummett. Mr. A. Flew.  
(b) Symposium: "When is a principle a moral  
principle?"  
Mrs. P. R. Foot. Mr. Jonathan Harrison.  
at 8 p.m. Symposium: "Can there be a private language?"  
Professor A. J. Ayer. Mr. R. Rhees.
- Sunday, 11th July, (a) Symposium: "Sensing and Observing".  
at 10 a.m. Mr. R. J. Hirst. Mr. R. Wollheim.  
(b) Symposium: "Language in Fiction".  
Miss M. Macdonald. Mr. M. Scriven.  
at 8 p.m. Symposium: "Pleasure".  
Professor G. Ryle. Professor W. B. Gallie.

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Overseas members are reminded that programmes and entry forms for the Joint Session are being sent automatically to members resident in Europe only. Any overseas member who expects to be in Europe in July and wishes to attend the Joint Session is asked to notify the Secretary, Professor K. W. Britton, King's College, Newcastle-on-Tyne, by *Air Mail*. He will then be sent the necessary papers.